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ART. I.—*Contributions to Meteorology, being Results derived from an examination of the United States Weather Maps and from other sources;* by ELIAS LOOMIS, Professor of Natural Philosophy in Yale College. Fourth paper. With Plate.

[Read before the National Academy of Sciences, Philadelphia, Nov. 2, 1875.]

Movement of Areas of high barometer.

HAVING determined the average direction and velocity of movement of areas of low barometer within the limits of the United States, I desired to make a similar determination respecting areas of high barometer. As I have in my possession only one Weather Map for each day, I have frequently found it difficult to follow the course of areas of high barometer from day to day, and have therefore confined my comparison to the monthly charts published by the U. S. chief signal officer. Among these I found three charts which gave the tracks of areas of high barometer for a month each, viz: Aug., 1873, Dec., 1874, and Jan., 1875. The following are the average courses and velocities of the areas of high barometer for these months:

MONTHS.	Course.	Velocity. Miles per hour.	High minus Low.	
			Course.	Velocity.
January,	E. 2° N.	21·0	7° S.	−5·7
August,	E. 41 S.	18·9	46 S.	+0·5
December,	E. 18 S.	28·5	25 S.	−0·8
Mean	E. 12 S.	24·8	21 S.	−1·2

The lower line shows the averages for the three months, regard being had to the number of cases in each month. Comparing these results with those given in my last article for areas of low barometer (this Jour., vol. x, p. 5), we find that for each month the course of high barometer is more southerly than low barometer. These differences are shown in column fourth. Column fifth shows the differences in velocity for each month.

These observations indicate that while the average track of storms east of the Rocky Mountains, across the United States, is nine degrees to the north of east, areas of high barometer advance toward a point several degrees south of east, and with a velocity somewhat less than the former.

Monthly minima of temperature.

In a former article (this Jour., vol. ix, p. 7) I gave a table showing the lowest temperature observed at New Haven for each month during a period of three years, together with the height of the barometer, direction of the wind, and degree of cloudiness for the corresponding dates, and I expressed the opinion that these low temperatures are due in part to the descent of cold air within an area of high barometer. Inasmuch as some persons ascribe these low temperatures to a flow of air from a higher latitude, it has appeared to me that it would be instructive to study the same phenomenon at a locality where a current of air from a colder latitude is impossible; and such a locality must be found at the point of minimum temperature for the northern hemisphere. Now according to Dove's charts; during the winter months Jakutsk, in Siberia, lat. 62° 2′ N., is situated very near the center of greatest cold for the northern hemisphere. I have, therefore, sought for a complete meteorological journal at this station, and have found it in Middendorff's Sibirische Reise, Band I, pp. 28–49, extending from Sept., 1844, to June, 1846. The following table shows the results obtained for each month of this period.

Column first shows the date of the lowest temperature for each month; column second shows the lowest temperature for each month in degrees of Reaumur; column third shows the direction of the wind, and column fourth shows the force of the wind; column fifth shows whether the sky was clear or overcast; column sixth shows the height of the barometer expressed in Russian half lines; column seventh shows the mean height of the barometer for each month; column eighth shows how much the observed height of the barometer differed from the mean height expressed in English inches (one-half line=0·05 inch English).

From this table it will be seen that the monthly minima of temperature are almost entirely independent of the direction of

the wind. A northerly direction occurs 10 times; southerly, 6 times; easterly 7 times; and westerly, 5 times. In every instance (except one) these winds were faint, and generally *very faint.* In two-thirds of the cases the sky was entirely clear, and in only two cases was the sky entirely overcast. In three-fourths of the cases the barometer stood above its mean height.

Lowest temperature for each month at Jakutsk, Siberia, for the years 1844, 5 *and* 6.

Date of minimum.	Minimum temp'e.	Wind direction.	Wind force.	Face of sky.	Russian Half Lines. bar.	Russian Half Lines. m'n bar	Differ. Eng. Inches.
1844, Sept. 30	−3°·1	N. W.	very faint.	overcast.	597·9	594·2	+0·19
Oct. 31	−26·4	Calm.		clear.	599·2	593·4	+0·29
Nov. 29	−40·1	N. N. W.	very faint.	clear.	601·5	596·5	+0·25
Dec. 18	−44·8	Calm.		partly clear.	595·1	601·2	−0·30
1845, Jan. 16	−44·4	S. S. W.	very faint.	clear.	596·6	597·9	−0·06
Feb. 6	−40·7	Calm.		clear.	601·3	599·1	+0·11
March 7	−31·9	N.	very faint.	clear.	601·2	594·9	+0·31
April 5	−20·5	S.	very faint.	clear.	601·9	592·4	+0·48
May 13	− 7·8	W.	strong.	overcast.	591·8	589·6	+0·11
June 9	− 1·3	E.	faint.	clear.	584·4	587·3	−0·14
July 30	+ 4·9	N. E.	very faint.	clear	593·5	587·6	+0·30
Aug. 21	− 0·7	S.	very faint.	clear.	589·6	588·7	+0·05
Sept. 26	− 5·3	Calm.		partly clear.	596·2	594·2	+0·10
Oct. 31	−25·5	N. E.	faint.	partly clear.	597·4	593·4	+0·20
Nov. 23	−39·8	N.	very faint.	clear.	603·9	596·5	+0·37
Dec. 25	−41·1	N. E.	very faint.	clear.	604·6	601·2	+0·17
1846, Jan. 10	−41·9	N. N. E.	faint.	partly clear.	595·9	597·9	−0·10
Feb. 2	−43·6	N. N. E.	faint.	partly clear.	596·1	599·1	−0·15
Mar. 14	−28·0	S. E.	faint.	clear.	599·5	594·9	+0·23
April 10	−16·8	S.	very faint.	clear.	596·0	592·4	+0·18
May 3	−17·4	N.	faint.	clear.	596·3	589·6	+0·33
June 9	+ 2·4	W. S. W.	faint.	mostly overcast.	582·4	587·3	−0·24

In all these particulars, except the direction of the wind, the phenomena at Jakutsk are quite similar to those observed at New Haven. So far as I have yet learned it is true universally that periods of unusual cold are generally accompanied by a barometer above the mean. Now it has been shown (this Jour., vol. ix, p. 2) that within areas of high barometer the motion of the air is *outward* from the center of this area, and therefore there must be a downward motion to supply the air flowing outward. In other words, it must be regarded as an observed fact that periods of unusual cold are generally accompanied by *a descent of air from the upper regions of the atmosphere.*

Influence of Winds on the temperature, moisture and pressure of the atmosphere.

In order to determine the influence of winds upon the temperature, etc., of the atmosphere, I selected the Meteorological observations made at Girard College, Philadelphia, from 1840

to 1845. A large sheet of paper was ruled with 16 vertical columns, and at the head of these columns were placed the letters N.; N.N.E.; N.E., etc., for the directions of the wind. I first took the observations for the three winter months, and beginning with Dec. 1, 1840, found that at the first observation the wind was N.W. The temperature at this observation was compared with the mean temperature of that month for the same hour, and the difference (with its algebraic sign) was placed in the column headed N.W. I proceeded in like manner with each succeeding observation during the winter months for the whole period of five years. The average of all the numbers in each column was taken and the results are shown in column second of the following table. These numbers clearly indicate that the coldest winds are from the neighborhood of the N.W., and the warmest winds from the neighborhood of the S.E. As, however, the progress of the numbers is somewhat irregular, I have taken the average of each successive three numbers in this column and placed the results in column third. These numbers show a remarkable regularity, indicating a minimum with a N.W. wind, and increasing thence uninterruptedly to a maximum with a S.E. wind, and thence decreasing uninterruptedly to the minimum. The entire range of the numbers in column third is 8°·46 Fahr., while the extreme range in column second is 10°·18.

Influence of winds on the temperature, moisture and pressure of the atmosphere.

Direction of wind.	Thermometer at Philadelphia. Fahrenheit.				Force of vapor at Philadelphia. Inches of mercury.				Barometer at Philadelphia. 29 inches plus.			
	Winter means.	Winter aver'd.	Sum'er means.	Sum'er aver'd.	Winter means.	Winter aver'd.	Sum'er means.	Sum'er aver'd.	Winter means.	Winter aver'd.	Sum'er means.	Sum'er aver'd
	°	°	°									
N.	−1·57	−1·53	−3·69	−3·23	−·007	−·005	−·096	−·085	1·039	1·005	·950	·933
N. N. E.	−0·45	−0·25	−2·78	−2·85	+·005	+·003	−·072	−·072	·986	1·013	·937	·956
N. E.	+1·28	+0·40	−2·07	−2·51	+·012	+·005	−·047	−·055	1·014	1·038	·980	·975
E. N. E.	+0·36	+1·00	−2·68	−2·21	−·003	+·011	−·046	−·040	1·113	1·033	1·008	1·001
E.	+1·37	+2·66	−1·87	−1·82	+·023	+·025	−·026	−·029	·973	1·018	1·014	1·018
E. S. E.	+6·26	+4·25	−0·90	−1·15	+·055	+·043	−·016	−·004	·967	·953	1·033	1·014
S. E.	+5·11	+5·94	−0·66	−0·24	+·051	+·060	+·031	+·023	·918	·913	·994	·978
S. S. E.	+6·46	+5·69	+0·83	+0·55	+·075	+·059	+·054	+·046	·857	·898	·906	·934
S.	+5·50	+5·00	+1·48	+1·68	+·052	+·054	+·052	+·057	·920	·889	·901	·917
S. S. W.	+3·05	+3·88	+2·72	+2·48	+·036	+·029	+·066	+·061	·891	·900	·943	·912
S. W.	+3·09	+2·47	+3·24	+2·51	+·029	+·029	+·066	+·056	·890	·885	·893	·906
W. S. W.	+1·26	+1·08	+1·57	+2·42	+·023	+·007	+·036	+·038	·874	·911	·882	·896
W.	−1·12	−1·19	+2·44	+1·60	−·032	−·016	+·012	+·005	·968	·923	·914	·881
W. N. W.	−3·72	−2·03	+0·80	+0·10	−·039	−·032	−·034	−·034	·926	·961	·846	·882
N. W.	−1·26	−2·52	−2·94	−1·79	−·025	−·025	−·081	−·067	·989	·968	·887	·882
N. N. W.	−2·57	−1·80	−3·22	−3·28	−·012	−·015	−·086	−·088	·989	1·006	·912	·916

I proceeded in a similar manner with the observations for the three summer months and the results are shown in column

fourth. The average of each successive three numbers in this column was taken and the results are shown in column fifth.

In order to determine the influence of the winds on the moisture of the air I took the observations of the force of vapor (expressed in inches of mercury) and compared each observation with the mean force for that month, and the given hour. The results for the three winter months are shown in column sixth of the preceding table, and the averaged numbers are shown in column seventh. The results for the three summer months, obtained in like manner, are shown in columns eighth and ninth.

In order to determine the influence of the wind on the pressure of the air, I placed each observation of the barometer in the column having the observed wind at the top, and took the averages of the numbers in each column. The result for the three winter months (subtracting 29 inches) are shown in column tenth and the averaged numbers in column eleventh. Similar results for the three summer months are shown in columns twelve and thirteen.

On comparing the numbers in this table we see that in winter at Philadelphia the lowest temperature occurs with a wind from the N.W., and in summer with a wind from a point about 15° west of north.

Now if we suppose a mass of air to be transferred from a higher latitude to a lower, we should expect that its relative temperature would be the lowest when it moved in a direction perpendicular to the isothermal lines. Observing this rule we should conclude that in winter the coldest wind at Philadelphia must come from a quarter about 15° west of north, provided it commenced its motion from any place within 600 miles from Philadelphia. But if it came from a distance of over 1000 miles from Philadelphia then the coldest wind would come from a point 30° west of north. But the coldest wind actually comes from a point 45° west of north; that is, at Philadelphia in winter the coldest wind blows from a point 15° more westerly than the coldest region about Philadelphia.

In summer, if we extend the comparison to a distance of 1000 miles from Philadelphia, we shall find the coldest region to lie in a N.E. direction; but if we confine ourselves to a radius not exceeding 600 miles, we shall find nearly the same temperature prevailing in all directions between the limits of N.E. and N. 25° W. But the coldest wind is observed to blow from a point N. 15° W., which lies within the limits above determined. On the whole, we conclude that at Philadelphia the coldest wind comes very nearly from the coldest region within a distance of from 500 to 1000 miles from Philadelphia, with a suspicion, however, that the former is a few degrees more westerly than the latter.

In winter the warmest wind at Philadelphia is found to blow from a direction S. 40° E., while in summer it blows from the S.W. The former direction takes us to the Gulf Stream at about its nearest point, and at a distance of 250 miles. In summer the warmest region within 400 or 500 miles of Philadelphia lies in a direction S. 30° W., while the warmest wind blows from a point 15° more westerly. On the whole, the observations indicate that both the warmest and coldest winds at Philadelphia blow pretty nearly from the regions of greatest heat and cold, but there is reason to suspect that these directions are not quite coincident.

From the table of monthly minima of temperature at New Haven given in my former article, (this Jour., vol. ix, p. 7) it will be seen that the average monthly minimum is 25° below the mean temperature of the corresponding month. The table last given shows that a small portion of this effect (viz. 5°) may be ascribed to the influence of the direction of the wind, but there remains unexplained *four-fifths* of the whole effect which is to be ascribed to the influence of other causes.

The preceding table shows that both in summer and winter the force of vapor at Philadelphia is greatest with the same wind which brings the highest temperature; and it is lowest with the wind which brings the lowest temperature. The deviations from this rule are so small as to render it probable that the discrepancies would entirely disappear in the means of a long series of observations.

Since cold air has a greater density than warm air, and dry air has a greater density than vapor of water, it might be expected that the wind which brings the lowest temperature and the least vapor, would bring the highest pressure. We see, however, from the preceding table that such is not the case. In winter the highest pressure comes with a wind from the N.E., or perhaps N. 55° E.; while in summer the highest pressure comes with an east wind, which directions are distant more than 90° from the coldest quarter of the horizon. So, also, in winter, the lowest pressure comes with a S.W. wind, and in summer with a west wind, both of which directions are quite distant from the warmest quarter of the horizon. It seems probable that the excess of pressure which accompanies an easterly or N.E, wind is but the result of the high barometer which usually precedes a N.E. storm.

Diurnal inequality in the rain-fall.

In my former article (this Jour., vol. x, p. 3) I noticed a diurnal inequality in the progress of storms and was hence led to infer that there must be a diurnal inequality in the fall of

rain. In searching for observations to test this conclusion, I found a decided diurnal inequality in the rain-fall at Philadelphia showing a maximum about 6 P. M. and a minimum at 3 A. M. The observations made by the United States Signal Service did not show any decided diurnal inequality, owing, perhaps, to their including only three daily observations. In a series of hourly observations made at seven stations in Great Britain I found evidence of two daily maxima and two daily minima.

Since the publication of my former article I have found in Kreil's Klimatologie von Böhmen the results of ten years' observations at Prag, lat. 50° 5′, which show a decided diurnal inequality, having a maximum about 4 P. M. and a minimum about 7 A. M., with a second maximum which is less distinctly marked. The following table shows the average annual rain-fall at Prag as deduced from observations from 1850 to 1859 expressed in Paris lines:

Rainfall at Prag, Austria.

Hour.	Rain-fall.	Hour.	Rain-fall.
Midnight.	6·864	Noon.	7·063
1 A. M.	6·507	1 P. M.	8·901
2	5·986	2	8·837
3	6·514	3	10·207
4	6·313	4	10·778
5	6·771	5	10·845
6	5·637	6	8·835
7	5·049	7	8·211
8	6·949	8	6·927
9	5·722	9	6·374
10	5·798	10	6·953
11	6·072	11	6·542

These numbers follow a law bearing a close resemblance to that of the Philadelphia observations, and lead us to presume that a similar law must prevail in the fall of rain over a considerable portion of the United States.

I have received the hourly observations of rain-fall at seven stations of Great Britain complete for the year 1874. At most of the stations there are evidently two periods of maximum rain-fall, but the time of maximum appears to depend very much upon local circumstances.

Comparison of storm paths in America and Europe.

In comparing the tracks followed by storms in America and Europe I have depended chiefly upon the following materials:

1. The daily United States Signal Service maps from 1871 to 1875, and especially the monthly maps showing the tracks of storm centers.

2. Atlas des mouvements generaux de l'atmosphere, redigé par l'observatoire Imperial de Paris, embracing 18 months, from June, 1864, to Dec. 1865.

3. Cartes synoptiques journalieres construites par N. Hoffmeyer, Copenhagen, embracing 9 months, from Dec. 1873 to Aug. 1874.

In order to determine what may be called the average track of storm centers in the United States, I ruled a large sheet of paper with several vertical columns headed 122°, 117°, 107°, etc., these numbers denoting degrees of longitude from Greenwich. I then took one of the monthly maps showing the tracks of storm centers, and following each of the tracks in succession determined in what latitude it crossed the meridians indicated at the top of the table, and the results were set down in the appropriate column. I proceeded in the same manner with each of the monthly maps and then took the average of all the numbers in each column. The results are shown in the first two columns of the following table; where column first shows the meridians of longitude from Greenwich, and column second shows the average latitude in which each of these meridians is intersected by the storm paths. The curve thus determined is traced on the accompanying chart and passes over the center of Lake Erie. It will be seen that the average direction of storm paths is not the same on all meridians. The directions given in my former article (see this Jour., vol. x, p. 1) must be understood to be the average direction of storm paths for the region covered by the United States observations; and this represents pretty nearly the mean direction for a place whose latitude is 42½° and longitude 83½° W., which is nearly the position of Detroit, Michigan.

Average direction of storm-paths.

U. S. Weather Maps.		Paris Maps.		Danish Maps.		Mean of Par. and Dan.	
Long. from Greenwi'h.	Latitude	Longitude from Paris.	Latitude.	Longitude from Paris.	Latitude.	Longitude from Paris.	Latitude.
122° W.	45·8°	60°—45° W.	46·2°	60°—50° W.	53·7°	55° W.	50·0°
117	46·9	45 —30	46·5	50 —40	59·5	45	52·9
107	44 7	30 —15	47·5	40 —30	61·8	35	54·2
97	42·2	15 — 0	53·0	30 —20	62·6	25	54·9
87	42·5	0 —15 E.	54·5	20 —10	61·5	15	55·6
77	42·6	15 —30	56·9	10 — 0	63·1	5 W.	58·3
67	45·2	30 —45	51·4	0 —10 E.	58·9	5 E.	56·7
62	45·9			10 —20	57·8	15	56·9
				20 —30	61·6	25	59·0
				30 —40	61·2	35	56·8
				40 —50	58·5	45	55·0
				50 —60 E.	55·3	55 E.	53·3

I proceeded in a somewhat similar manner with the Paris maps, but since these maps do not generally exhibit lines

drawn so as to show the tracks of storms from day to day, I placed in one column the latitudes of all the storm centers between the meridians of 60° and 45° W. from Paris; in a second column I placed those between the meridians of 45° and 30° W., and so on for each 15° of longitude. I then took the average of all the latitudes in each of the columns. The result is shown in columns third and fourth of the preceding table, and the path thus determined is traced on the accompanying chart. This path passes over Dublin, and will be seen to form a natural continuation of the track deduced from the American observations. This result is doubtless accidental and is due to the fact that near the American coast the observations from which the Paris maps were constructed were derived from a region extending but little north of the stations of the United States Signal Service. If the Paris maps had included observations from Labrador and Greenland the average track of the storms represented would have been more northerly than it now is.

I proceeded in a similar manner with Hoffmeyer's charts except that the meridians were selected at intervals of 10°, and the results are shown in columns fifth and sixth. The path thus determined lies several degrees north of that previously determined, and this arises from the fact that the maps exhibit the results of observations made in Greenland and Iceland as well as from more southern latitudes. In order to deduce an average result from the French and Danish maps I have combined them in a single series, and the result is shown in columns seventh and eighth of the preceding table. The path thus determined is traced on the accompanying chart and passes through the northern extremity of Scotland.

In order to show the connection between storm paths and the mean height of the barometer, I have drawn upon the same chart two other barometric lines. The mean height of the barometer at the level of the sea varies with the latitude of the place. On the Atlantic ocean at the equator the mean height of the barometer is about thirty inches. If from this point we travel northward the pressure increases, and in latitude 30° becomes about 30·2 inches. Thence the pressure diminishes to 29·6 inches near latitude 70°, from which point the pressure slightly increases as we advance northward. A somewhat similar result takes place in going from the equator to the North Pole upon any meridian, but the maximum pressure is not the same under all meridians, and the same is true of the minimum pressure. The undulating line near the bottom of the accompanying chart shows the line of the greatest mean pressure varying on different meridians from about 30 inches to 30·2 inches. The undulating line near the top of the chart

shows the line of the least mean pressure, being about 29·6 inches on the meridian of Greenwich and increasing somewhat as we proceed either east or west from that meridian. These lines are drawn chiefly from data collected by Alexander Buchan. (See Edinburgh Phil. Trans, vol. xxv.)

We perceive then that at all places near the southern margin of the chart the mean pressure of the atmosphere is greater than it is further northward, and this is generally sufficient to cause an average surface wind from south to north although the wind advances from a warmer to a colder region. On the other hand, at places near the northern margin of the chart the mean pressure of the atmosphere is somewhat greater than it is further south, and this force combined with a lower mean temperature causes a surface wind from north to south. Here then are permanent causes producing winds from opposite directions near the upper and lower portions of the chart, and these must be a permanent source of storms independent of those inequalities of pressure which arise from causes of a more local nature.

The average path of storms in their progress from America to Europe is apparently modified by the line of greatest mean pressure. This line has a more northerly position in Europe than it has in America, and this may be the reason why storm tracks generally bend northward in advancing from America to Europe. There are some minor particulars in which storm paths are apparently modified by the line of greatest mean pressure; but instead of attaching importance to coincidences which may prove to be accidental, it is more prudent to wait and see if these peculiarities are confirmed by further observations.

Oscillations of the barometer in different latitudes.

For the purpose of determining in what region of the globe the oscillations of the barometer are the greatest, I have prepared a table showing the mean monthly oscillation of the barometer at as many stations as possible in high northern latitudes. A few of the numbers in the following table are derived from Kaemtz Meteorology, edited by C. V. Walker, p. 297. The other numbers have been collected by myself from various sources which are indicated in the last column, and some of the results have required a careful discussion of many years' observations. Column fourth shows the average monthly range of the barometer for the three winter months, and column fifth shows the same for the three summer months expressed in English inches.

As some of these numbers depend upon observations of only one year, and therefore do not represent mean values very ac-

curately, I have endeavored to combine them so as to obtain a few normal values. I combined all the observations north of latitude 70° in one general average, and all the observations between latitudes 60° and 70° in a second average. I then divided the observations of Kaemtz (Met., p. 298) into similar groups, each embracing ten degrees of latitude, viz., 60°—50°; 50°—40°, etc., and thus obtained the normal values shown in the table at the bottom of this page.

Mean monthly oscillation of the barometer for winter and summer.

Place.	Latitude.	Longitude.	Range of bar. Win'r	Sum'r	Authority.
	° ′	° ′			
Van Rensselaer Harbor,	78 37	70 53 W.	1·483	0·727	Kane's obs. reduced by Schott.
Northumberland Sound,	76 52	97 0 W.	1·116	0·728	Belcher Expedition, 1852–4.
Wellington Channel,	75 31	92 10 W.	1·130	0·587	" "
Melville Island,	74 47	110 48 W.	1·220	0·693	Parry's first voyage.
Upernivik, Greenland.	72 48	55 53 W.	1·451	0·697	Collectanea Met., 5 years obs.
Vardoe, Norway,	70 22	31 7 E.	1·776	0·937	Met. Iag. i Norge, 2 years obs.
Boothia Felix,	70 3	91 52 W.	1·231	0·899	Ross 2d Arctic Expedition, 2¼ years.
Alten, Finmark,	69 58	23 2 E.	1·424	0·907	Br. Ass. Rep., 1848–50, 3 years.
Kaafiord, Finmark,	69 56	23 5 E.	1·496	0·901	Gaimard Met., v. 2, p. 451, 3 years.
Tromsoe, Norway,	69 39	18 58 E.	1·721	0·791	Met. Iag. i Norge, 1 year obs.
Jacobshaven, Greenland,	69 12	51 0 W.	1·496	0·906	Collectanea Met., 10 years obs.
Bodoe, Norway,	67 17	14 24 E.	1·913	0·921	Met. Iag. i Norge, 2 years obs.
Ft. Confidence, Br. N. A.,	66 54	118 49 W.	1·315	...	Athabasca obs., p. 355, 7 months.
Torneo, Finland,	65 51	24 14 E.	1·512	0·851	Kaemtz Met., p. 298.
Haparanda, Sweden,	65 51	24 11 E.	1·675	0·892	Met. Iakttagelser, 1859–69.
Godthaab, Greenland,	64 10	51 53 W.	1·409	0·685	Collectanea Met., 3 years obs.
Naes, Iceland,	64 9	22 0 W.	1·639	0·971	Observationes Met., 15 years obs.
Umea, Sweden,	63 50	20 18 E.	1·555	0·868	Kaemtz Met., p. 298.
Christiansund, Norway,	63 7	7 45 E.	1·642	0·929	Met. Iagttagelser i Norge, 9 years obs.
Hernosand, Sweden,	62 38	17 57 E.	1·644	0·896	Met. Iakttagelser, 1859–69.
Aalesund, Norway,	62 29	6 9 E.	1·658	0·949	Met. Iag. i Norge, 9 years obs.
Dovre, Norway,	62 5	9 7 E.	1·559	0·870	Met. Iag. i Norge, 5 years obs.
Jakoutsk, Siberia,	62 2	129 42 E.	1·012	0·803	Kaemtz Met., p. 298.
Fahlun, Sweden,	60 36	15 37 E.	1·640	0·835	Met. Iakttagelser, 1859–69.
Abo, Russia,	60 27	22 19 E.	1·465	0·778	Kaemtz Met., p. 298.
Bergen, Norway,	60 24	5 18 E.	1·654	0·904	Kaemtz Met., p. 298 & Met. Iag. i Norge.

Monthly oscillation of the barometer (normal values).

Latitude.	Winter inch.	Summer inch.	Winter comp.	Winter O—C.	Summer comp.	Summer O—C.
° ′						
6 12	0·110	0·106	0·125	—0·015	0·115	—0·009
15 40	0·198	0·164	0·234	— ·036	·169	— ·005
24 10	0·424	0·236	0·404	+ ·020	·255	— ·019
34 57	0·664	0·362	0·690	— ·026	·398	— ·036
46 2	1·068	0·556	1·029	+ ·039	·568	— ·012
54 13	1·315	0·744	1·280	+ ·035	·694	+ ·050
64 46	1·549	0·869	1·566	— ·017	·837	+ ·032
74 9	1·344	0·753	1·757	— ·413	·933	— ·180

The winter oscillations up to lat. 65° are pretty well represented by the formula:

$$W = 1{\cdot}890 \sin^2 l + 0{\cdot}104 \cos^2 l,$$

and the summer oscillations by the formula:

$$S = 1{\cdot}000 \sin^2 l + 0.104 \cos^2 l.$$

The differences between the observed and computed values are shown in columns fifth and seventh in the above table. These results indicate a steady increase in the mean monthly oscillation up to about lat. 65°, and from that point the oscillation diminishes as we proceed northward. The term $0{\cdot}104 \cos^2 l$ represents approximately the diurnal oscillation of the barometer, and the

Storms on the Atlantic Ocean by Maury's Charts.

	80°	75°	70°	65°	60°	55°	50°	45°	40°	35°	30°	25°	20°	15°	10°	5°	W.	0°
60°												60	102	123	117	78	30	
												16	38	35	31	12	3	
												27	37	28	27	15	10	
55																		
									150	420	510	694	850	932	1260	1393	583	
									57	111	140	169	159	117	152	133	46	
									38	26	27	24	19	12	12	10	8	
50																		
						126	288	919	1242	1570	1740	1627	1539	1478	1312	920	313	
						11	28	121	209	369	277	242	165	160	156	85	13	
						9	10	13	17	24	16	15	11	11	12	9	4	
45																		
		1820	3249	2544	2679	2419	1863	1581	1119	732	396	269	168	128	67	0		
		126	260	266	269	241	280	234	127	66	58	44	16	9	1			
		7	8	11	10	10	15	15	11	9	15	16	9	8	1			
40																		
	243	4193	2974	1797	1393	1100	773	480	349	242	341	268	302	340	334	225		
	8	607	475	393	177	115	62	28	27	23	35	5	24	7	9	14		
	4	14	16	22	13	10	8	6	8	9	10	2	8	2	3	6		
35																		
	1534	2265	1645	766	723	860	986	893	747	392	175	129	223	77	3			
	126	231	137	65	72	71	60	48	27	25	4	0	9	0	0			
	8	10	8	9	10	8	6	5	3	6	2	0	4	0	0			
30																		
	1945	1393	1137	948	394	351	564	726	958	663	209	153	87					
	81	30	57	17	17	6	5	9	61	6	8	0	6					
	4	2	5	2	4	2	1	1	6	1	4	0	7					
25																		
	316	380	262	650	637	267	262	452	806	664	338	136	15					
	12	9	4	7	6	6	9	18	4	25	0	0	0					
	4	2	2	1	1	2	3	4	0	4	0	0	0					
20																		
	243	320	152	183	459	541	326	302	449	711	638	159	6					
	4	2	0	0	1	4	1	10	6	13	8	0	0					
	2	1	0	0	0	1	0	3	1	2	1	0	0					
15																		
	65	0	53	53	96	387	594	508	415	667	835	622	225					
	0	0	1	0	1	0	1	0	3	9	21	0	0					
	0	0	2	0	1	0	0	0	1	1	3	0	0					
10																		
						23	0	289	668	632	739	1667	1109	483	716	80	70	
						0	0	0	0	0	3	5	0	0	0	0	0	
						0	0	0	0	0	0	0	0	0	0	0	0	
5																		
							0	95	421	613	1004	2004	1262	335	107	362	233	
							0	0	0	0	0	3	0	0	0	1	0	
0							0	0	0	0	0	0	0	0	0	0	0	

other term of the formulas varies as the square of the sine of the latitude, and this law of increase holds pretty closely up to about latitude 65°. That part of the barometric oscillation represented by the first term of the formula is the effect of storms, and the oscillation diminishes within the Arctic circle.

These results seem to indicate that in the Northern Hemisphere, storms increase in frequency as we proceed northward as far as latitude 60° and perhaps somewhat farther. The same result is shown by Maury's storm chart of the North Atlantic. The preceding table presents a summary of the results of this chart. The ocean is divided into squares by parallels of latitude drawn at intervals of five degrees from each other, and meridians of longitude at intervals of five degrees Each square of the preceding table contains three numbers. The first shows the number of observations within the given square, each observation representing a period of eight hours. The second shows the number of gales reported, and the third is the average number of gales occurring in a hundred observations. Thus in the square included between the parallels of 40° and 45° of north latitude, and between the meridians of 45° and 50° west longitude from Greenwich, the first number is 1863, which shows the number of observations obtained in that square. The second number is 280, which denotes the number of gales reported; the third number is 15, which denotes that the number of gales was 15 per cent of the whole number of observations. An inspection of this table will show that on each meridian the frequency of gales increases with the latitude up to the highest latitude from which observations are reported.

Storms traced across the Atlantic Ocean.

When storms from the American continent enter upon the Atlantic Ocean they generally undergo important changes in a few days and are frequently merged in other storms which appear to originate over the ocean, so that we can seldom identify a storm in its course entirely across the Atlantic. The following are the only cases I have found on the French and Danish charts (embracing a period of 27 months) in which storms can be pretty distinctly traced across the Atlantic.

1. Nov. 30—Dec. 11, 1864. A storm traced from Newfoundland to Ireland.
2. April 20—May 3, 1865, traced from Labrador to Ireland.
3. May 26—29, 1865, from Gulf St. Lawrence to Ireland.
4. Oct. 2—10, 1865, from Cape Cod to Ireland.
5. Oct. 11—17, 1865, from Newfoundland to Ireland.
6. March 1—5, 1874, from Hudson Bay to North Cape.
7. April 14—17, 1874, from Hudson Bay to Norway.

8. April 16—23, 1874, from Gulf St. Lawrence to Norway.
9. May 23—30, 1874, from Gulf St. Lawrence to Norway.
10. Aug. 1—4, 1874, from Gulf of St. Lawrence to North Cape.
11. Aug. 12—17, 1874, from Hudson Bay to Norway.

If the observations each day were sufficiently numerous to show the isobaric curves for every part of the Atlantic Ocean, doubtless many more storms might be traced from America to Europe, but it is presumed that such cases do not occur on an average more than once or twice a month. The storms of Europe generally have their origin considerably east of the American Continent and soon become so violent as to draw within their influence any small barometric depression which started from America.

Velocity of Ocean Storms.

The average velocity of storms upon the Atlantic Ocean as deduced from 134 cases on the French maps is 19·3 miles per hour; the velocity deduced from 49 cases on the Danish maps is 20·3 miles per hour; giving an average of 19·6 miles per hour from both series of maps. The average velocity for the storms of the United States as deduced from 485 cases is 26 miles per hour. From a considerable number of cases in Europe, Prof. Mohn has deduced an average velocity of 26·7 miles per hour. These numbers indicate that storms travel with less velocity over the Atlantic Ocean than they do over the Continents of America and Europe; and it seems to follow that the progressive movement of a storm is not the result of a *simple drifting* of the atmosphere; for it seems probable that the average progress of the atmosphere in an easterly direction is as rapid over the Atlantic Ocean as it is over North America.

Storms of Jan. 29—Feb. 8, 1870, on the Atlantic Ocean.

A succession of storms of unusual severity passed over the Atlantic, between Jan. 29 and Feb. 8, 1870, an account of which has been published by Capt. Henry Toynbee, of the London Meteorological office. On the 30th of January an area of low barometer prevailed near Nova Scotia; on the 31st it was east of Newfoundland; and on the 1st of February it was merged in another storm which had prevailed for several days on its eastern side. On the 2d of February a second storm center appeared near Newfoundland; on the 3rd it had advanced east about 700 miles; and on the 4th it became merged in another storm off the Irish coast.

On the morning of the 5th a third storm appeared near the center of the Atlantic, which must have developed with unusual rapidity, since on the preceding day, observations had indicated no great atmospheric disturbance in that neighborhood.

The isobar of 29 inches is shown on the accompanying chart. On the afternoon of the same day, this storm blended with another storm on its eastern side, and there resulted one of the most violent hurricanes ever experienced on the Atlantic Ocean. At 6 P. M. the barometer fell to 27·33 inches, which Capt. Toynbee pronounces the lowest ever observed on this part of the Atlantic. The accompanying chart represents the isobar of 29 inches on the morning of the 6th, when the diameter of this curve was over 1000 miles, and the diameter of the isobar of 30 inches was over 2000 miles. During the next two days the storm advanced slowly towards the southeast, and its severity was much diminished. The accompanying chart shows the isobar 29·5 inches on the morning of Feb. 8th. During this interval of three days the center of the storm had moved only about 900 miles, showing an average velocity of about 12 miles per hour.

Application of Ferrel's formula.

In vol. viii of this Journal, p. 343, Prof. Ferrel has given a formula which enables us to compute the depression of the barometer resulting from a violent storm. If we divide the denominator of this formula by the number of inches in a mile, and suppose the wind to move in a circle, the formula becomes

$$G = \frac{v \sin l}{250} + \frac{v^2}{131r},$$

where G is expressed in inches, but v and r are expressed in miles. I have applied this formula to the storm of Feb. 5th, 1870, and the results are shown in the following table. Column first shows the isobars which have been selected as the basis of comparison; column second shows the radius of each isobar as nearly as can be determined from the observations of Capt. Toynbee's memoir; and column third shows the velocity of the wind in miles on each of these isobars. These velocities were obtained by taking the mean of the various observations corresponding to the barometric heights given in column first. These velocities were recorded in the numbers of Beaufort's scale (0–12) and were reduced to miles by the table in Scott's Met. Instruments, p. 58. Column fourth shows the gradient to 100 miles computed by the above formula, for points midway between the several isobars selected. If this gradient be supposed to be maintained for a distance equal to the distance between the isobars, it will show a change of barometric pressure about the same as that actually observed. For the inner circle, the computed gradient will represent the observed depression of the barometer if we suppose that near the center of the storm there was a considerable mass of air revolving with a diminished velocity.

Ex. 1.—*Storm of Feb.* 5, 1870, *Atlantic Ocean, lat.* 51° 3′ *N.*

Barometer. Inches.	Radius of Isobar. Miles.	Velocity, miles.	Gradient to 100 miles.
27·33	0	90	
28·00	60	66	1·79
28·50	200	59	·42
29·00	400	52	·25
29·50	680	45	·18
30.00	1020	38	·15

I have made a similar application of the formula to two violent cyclones of recent occurrence on the coast of the United States, and the results are shown below. In the Punta Rassa cyclone the assumed velocities 90 and 70 miles agree pretty well with the velocities actually observed; the velocities 50 and 35 miles are somewhat greater than the observations at the surface of the earth, but may be presumed to have been the velocities at a little elevation above the earth's surface. The velocities assumed for the Indianola cyclone are the velocities actually observed or estimated at Indianola.

Ex. 2.—*Storm of Oct.* 6, 1873, *Punta Rassa, lat.* 27° 0′.

Barometer.	Radius of Isobar.	Velocity.	Gradient to 100 miles.
28·40	0	90	
29·00	50	70	2·10
29·50	200	50	·33
30·00	650	35	·11

Ex. 3.—*Storm of Sept.* 16, 1875, *Indianola, lat.* 28° 31′.

Barometer.	Radius of Isobar.	Velocity.	Gradient to 100 miles.
28·90	0	90	
29·50	100	60	1·00
30.00	500	35	·15

The following is an example of a great inland storm of unusual severity. Column third shows the *greatest* velocity of the wind observed at any station near the corresponding isobars in column first, and column fourth shows the velocity assumed in computing the gradients in column fifth.

Ex. 4.—*Storm of Nov.* 18, 1873, *New England, lat.* 41°

Barometer inches.	Radius of Isobar. Miles.	Velocity of wind.		Gradient to 100 miles.
		Observed.	Assumed.	
28·60	100	50	56	
29·00	300	57	48	·24
29·50	700	27	40	·15
30·00	1200	28	30	·10

Stationary Storms.

When a storm center has crossed the United States and passed to Nova Scotia or Newfoundland, we often find on the United States Weather Maps for two or three subsequent days the word *low* on the northeast corner of the maps, seeming to indicate that the center of the storm remained during that period nearly stationary. The Danish maps (from Dec., 1873, to Aug., 1874,) show us that storms do sometimes remain nearly stationary for several days.

Case I. From the 5th to the 8th of March, 1874, a violent storm moved from New Mexico to the St. Lawrence valley. On the 9th the center of this storm was a little north of Halifax; on the 10th it was still near the same place; on the 11th it had moved northeast nearly 400 miles; on the 12th it had moved south about 200 miles; on the 13th it had moved north about 200 miles; on the 14th it had moved south about 200 miles; and on the 15th it moved northeast about 700 miles. Thus during five days (March 9–14) the center of the storm had advanced less than 350 miles, being an average motion of less than three miles an hour, and during the first four days the barometric depression was greater than it was on the 8th.

Case II. From April 26th to 30th, 1874, a storm moved across the United States from Colorado to the St. Lawrence valley. During the next day (May 1st) the storm was stationary; on the 2d it moved a little to the southeast; on the 3d it moved a little to the east; and on the 4th it reached St. Johns, Newfoundland. Thus in four days the center moved 775 miles, being an average rate of about eight miles an hour; and during the first half of this time the average movement scarcely exceeded four miles an hour.

In preparing the materials for this article, I have been assisted by Mr. Edward S. Cowles, a graduate of Yale College of the class of 1873.

ART. II.—*Studies on Magnetic Distribution*; by HENRY A. ROWLAND, of the Johns Hopkins University, Baltimore.

(Continued from page 458 of the last volume.)

V.

LET us now consider the case of that portion of the bar which is covered by the helix. First of all, when the helix is symmetrically placed on the rod, equations (5) and (6) will apply. As Q''_ε is the quantity which is usually taken to represent

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the distribution of magnetism, being nearly proportional to the "surface-density" of magnetism, I shall principally discuss it.

In the first place, then, this equation shows that the distribution of magnetism in a very elongated electro-magnet, and indeed of a steel magnet, does not change when pieces of soft iron bars of the same diameter as the magnet are placed against the poles, *provided that equal pieces are applied to both ends;* otherwise there is a change. This result would be modified by taking into account the variation of the permeability, &c.

Let us first consider the case where the rod projects out of the end of the helix, as in Tables V, VI and VII. By giving proper values to the constants we obtain the results given in the last columns of the table. The agreement with observation is in most cases very perfect. We also see the same variation of r that we before noticed in the rest of the curves, and we see that it is in just the direction theory would indicate from the change of μ.

In these tables we come to a very important subject, and one to which I called attention some years back, namely, the *change in the distribution when the magnetizing-force varies, and which is due to change of permeability.* The following tables and figures show this extremely well, and are from very long rods with a helix a foot long at their center, as in the last three tables. The bar in both these tables was ·19 inch in diameter and 5 feet long. The zero-point was at the center of the bar and of the helix. The tables give values of Q'_x for the magnetizing-forces which appear at the head of each column, and which are the tangents of the angles of deflection of the needles of a tangent-galvanometer. Table VIII. only gives the part covered by the helix. Both tables are from the mean of both ends of the bar.

TABLE VIII.

x.	Strength of magnetizing current.			
	·108.	·194.	·378.	·600.
0				
1	2·7	3·2	·7	·6
2			·9	·6
3	2·4	2·7	·9	·8
4	3·3	3·9	1·7	·8
5	4·0	6·0	4·0	3·2
6	5·7	8·7	9·3	14·7

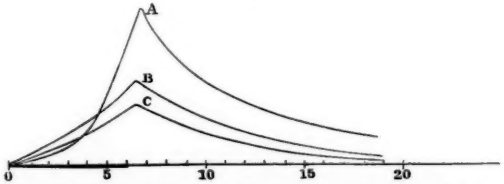
These experiments show in the most positive manner the effect we are considering, and we are impressed by them with the great complication introduced into magnetic distribution by the variable character of magnetic permeability.

TABLE IX.

x.	C. ·257.	B. ·363.	A. 1·303.
0			1·1
1	2·5	3·1	1·3
2			2·1
3	7·2	4·1	4·0
4		5·9	9·6
5	6·1	8·2	18·6
6	7·7	10·9	21·3
7	7·9	11·5	16·8
8	6·5	9·0	27·4
10	10·0	15·0	20·9
12	6·2	10·9	21·5
15	5·0	9·8	14·8
18	2·0	4·7	16·5
30	2·0	3·6	

In fig. 3 I have represented the distribution on half the bar as given in Table IX, the other half being of course similar. Here the greatest change is observed in the part covered by the helix, though there is also a great change in the other part.

3.



Plot of Table IX, showing surface-density for different values of the magnetizing-force.

These tables show that, as the magnetization of the bar increases, *at least beyond a certain point*, the curves on the part covered by the helix increase in steepness; and the figure even shows that near the middle of the helix an *increase of magnetizing-force may cause the surface-density to decrease;* and Table VIII. shows this even better. Should we calculate Q″, however, we should always find it to increase with the magnetizing-force in all cases. These effects can be shown also in the case where the bar does not extend beyond the helix, but not nearly so well as in this case, seeing that here Q″ can obtain a greater value.

Assuming that μ is variable, the formula indicates the same change that we observe; for as Q″ increases from zero upward, μ will first increase and then decrease; so that as we increase the magnetizing-force from zero upward, the curve should first decrease in steepness and then increase indefinitely in steepness.

In these tables the decrease of steepness is not very apparent, because the magnetization is always too great, and indeed on this account it is difficult to show it; but in Tables V, VI and VII. this action is shown to some extent by the values of r in the formulæ. The change of distribution with the helix arranged in this way at the center of the bar is greater than in almost every other case, because the magnetism of the bar Q″ can change greatly throughout the whole length of the helix, and thus the value of r be changed, and so the distribution become different.

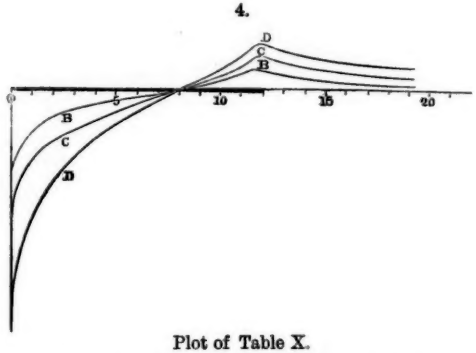
The next case of distribution which I shall consider is that of a very long rod having a helix wound closely around it for some distance at one end.

Table X. is from a bar 9 feet long with a helix wound for one foot along one end. The bar was ·25 inch in diameter. All except the first column is the sum of two results with the current in opposite directions, and after letting the bar stand for some time, as indeed was done in nearly every case. The first column contains twice the quantities observed, so as to compare with the others. The zero-point was at the end of the bar covered by the helix.

Table X.

x and L.	A. ·245.	B. ·360.	C. ·600.	D. 1·09.
0	+17·6	+29·4	+52·0	+108·7
1	+ 9·6	+16·8	+31·5	+ 60·1
2	+ 7·4	+13·1	+24·3	+ 45·8
3	+ 5·4	+ 9·8	+19·1	+ 34·1
4	+ 3·4	+ 7·2	+14·7	+ 22·8
5	+ 2·0	+ 4·6	+ 9·9	+ 16·0
6	+ 0·6	+ 2·4	+ 5·4	+ 9·6
7	− ·8	+ ·3	+ 1·2	+ ·6
8	− 1·8	− 1·6	− 2·1	− ·3
9	− 3·0	− 3·6	− 6·6	− 8·8
10	− 5·0	− 6·3	− 8·6	− 15·6
11	− 7·4	−10·0	−16·4	− 27·1
12	− 8·4	−10·0	−16·9	− 26·5
13	− 6·0	− 7·9	−14·5	− 22·6
14	− 5·2	− 7·0	−12·5	− 21·0
15	-----	− 5·3	−11·9	− 19·0
16	-----	− 9·4	−19·1	− 31·2
18	-----	− 5·3	−15·2	
20	-----	− 6·5	−19·3	
24	-----	− 5·6	− 6·0	
36	-----	− ·7	− 1·2	
48	-----			

The value of Q''_ε between 0 and 1 includes the lines of force passing out at the end of the bar, and is therefore too large.



Plot of Table X.

In fig. 4 we have a plot of the results found for this bar. The curves are such as we should expect from our theory except for the variations introduced by the causes which we have hitherto considered. Thus the sharp rise in the curve when near the end of the bar has already been explained in connection with Table III. A small portion of it, however, is due to those lines of induction which pass out through the end section of the bar, and in future experiments these should be estimated and allowed for. When considering surface-density we should also allow for the direct action of the helix, though this is always found too small except in very accurate experiments.

To estimate the shape of the curve theoretically in this case, let us take equation (4) once more, and in it make $s' = \infty$ and $s'' = \sqrt{RR'}$ which will make it apply to this. We shall then have $A' = -1$, and $A'' = \infty$.

Whence for the positive part of Q''_ε we have

$$Q''_\varepsilon = -\frac{\mathfrak{H}\Delta L}{2R'r}\{-1+2\varepsilon^{r(s-b)}-\varepsilon^{2r(s-b)}\} = \frac{\mathfrak{H}\Delta L}{2R'r}\{1-\varepsilon^{r(s-b)}\}^2;$$

and for the negative part

$$Q''_\varepsilon = -\frac{\mathfrak{H}\Delta L}{2R'r}(1+\varepsilon^{2r(s-b)})(1-\varepsilon^{-rs});$$

therefore the real value is

$$Q''_\varepsilon = -\frac{\mathfrak{H}\Delta L}{2R'r}(\varepsilon^{r(s-b)}(\varepsilon^{-rb}-2)+\varepsilon^{-rs}).$$

And if x is reckoned from the end of the rod, we have

$$Q''_\varepsilon = \frac{\mathfrak{H}\Delta L}{2R'r}\varepsilon^{-r(b+x)}\{1-2\varepsilon^{rb}+\varepsilon^{2rs}\}. \quad . \quad . \quad . \quad . \quad . \quad (10)$$

When $x=0$, this becomes

$$\frac{\mathfrak{H}\Delta L}{2R'r}\varepsilon^{-rb}(2-2\varepsilon^{rb});$$

and when $x=b$, it becomes

$$\frac{\mathfrak{H}\Delta L}{2R'r}\varepsilon^{-2rb}(1-2\varepsilon^{rb}+\varepsilon^{2rb}),$$

the ratio of which is

$$\tfrac{1}{2}(\varepsilon^{-rb}-1);$$

and this is the ratio of the values of Q''_e at the ends of the helix. When b is 12 inches, as in this case, we get the following values of this ratio:—

$r=$	·05.	·1.	·15.	·20.	·30.	∞.
$-\frac{1}{2}(\varepsilon^{-rb}-1)=$	·2256	·3494	·4173	·4546	·4863	·500
$\frac{-2}{\varepsilon^{-rb}-1}=$	4·43	2·86	2·40	2·20	2·06	2·00

To compare this with our experiments, let us plot Table X. once more, rejecting, however, the end observations and completing the curve by the eye, thus getting rid of the error introduced at this point. We then find for this ratio, according to the different curves,

B.	C.	D.
2·1	2·3	3·2

It is seen that these are all above the limit 2, as they should be, though it is possible that it may fall below in some cases owing to the variation of the permeability. As the magnetization increases, the values of the above ratio show that r decreases, as we should expect it to do from the variation of μ.

To find the neutral point in this case, we must have in formula (10)

$$\varepsilon^{2rx}=2\varepsilon^{rb}-1,$$

where x is the distance of the neutral point from the end. Making $b=12$, we have from this

$r=$	·05.	·10.	·15.	·20.	·30.	∞.
$x=$	10·1	8·96	8·31	7·89	7·39	6·00

By experiment we find that the neutral point is, in all the cases we have given in Table X, between 7·5 and 8·1 inches, which are quite near the points indicated by theory for the proper values of r, though we might expect curve D to pass through the point $x=9$, except for the disturbing causes we have all along considered.

Our formulæ, then, express the general facts of the distribution in this case with considerable accuracy.

These experiments and calculations show the change in distribution in an electro-magnet when we place a piece of iron

against *one pole only.* In an ordinary straight electro-magnet the neutral point is at the center. When a paramagnetic substance is placed against or near one end, the neutral point moves toward it; but if the substance is diamagnetic it moves from it.

The same thing will happen, though in a less degree, in the case of a steel magnet, so that its neutral point depends on external conditions as well as on internal.

We now come to practically the most interesting case of distribution, namely, that of a straight bar magnetized longitudinally either by a helix around it, or by placing it in a magnetic field parallel to the lines of force; we shall also see that this is the case of a steel magnet magnetized permanently. This case is the one considered by Biot (Traité de Phys., tome iii. p. 77) and Green (Mathematical Papers of the late George Green, p. 111, or Maxwell's "Treatise," art. 439), though they apply their formulæ more particularly to the case of steel magnets. Biot obtained his formula from the analogy of the magnet to a Zamboni pile or a tourmaline electrified by heat. Green obtained his for the case of a very long rod placed in a magnetic field parallel to the lines of force, and, in obtaining it, used a series of mathematical approximations whose physical meaning it is almost impossible to follow. Prof. Maxwell has criticised his method in the following terms ("Treatise," art. 439):—"Though some of the steps of this investigation are not rigorous, it is probable that the result represents roughly the actual magnetization in this most important case." From the theory which I have given in the first part of this paper we can deduce the physical meaning of Green's approximation, and these are included in the hypotheses there given, seeing that when my formula is applied to the special case considered by Green, it agrees with it where the permeability of the material is great. My formula is, however, far more general than Green's.

It is to Green that we owe the important remark that the distribution in a steel magnet may be nearly represented by the same formula that applies to electro-magnets.

As Green uses what is known as the surface-density of magnetization, let us first see how this quantity compares with those I have used.

Suppose that a long thin steel wire is so magnetized in the direction of its length that when broken up the pieces will have the same magnetic moment. While the rod is together, if we calculate its effect on exterior bodies, we shall see that the ends are the only portions which seem to act. Hence we may mathematically consider the whole action of the rod to be due to the distribution of an *imaginary* magnetic fluid over the ends

of the rod. As any case of magnetism can be represented by a proper combination of these rods, we see that all cases of this sort can be calculated on the supposition of there being two magnetic fluids distributed over the surfaces of the bodies, a unit quantity of which will repel another unit of like nature at a unit's distance with a unit of force. The surface-density at any point will then be the quantity of this fluid on a unit-surface at the given point, and the linear density along a rod will be the quantity along a unit of length, supposing the density the same as at the given point.

Where we use induced currents to measure magnetism we measure the number of lines of force, or rather induction, cut by the wire, and the natural unit used is the number of lines of a unit-field which will pass through a unit-surface placed perpendicular to the lines of force. The unit-pole produces a unit-field at a unit's distance; hence the number of lines of force coming from the unit-pole is 4π, and the linear density is

$$\lambda = \frac{Q_\varepsilon}{4\pi\Delta L}, \quad . \quad . \quad . \quad . \quad . \quad (11)$$

and the surface-density

$$\delta = \frac{Q_\varepsilon}{4\pi^2 d\Delta L}. \quad . \quad . \quad . \quad . \quad (12)$$

These really apply only to steel magnets; but as in the case of electro-magnets the action of the helix is very small compared with that of the iron, especially when it is very long and the iron soft,* we can apply these to the cases we consider.

Transforming Green's into my notation, it gives

$$\lambda = \left(\frac{\pi d^2}{4}\right) \mathfrak{H} \varkappa r \frac{\varepsilon^{r(b-s)} - \varepsilon^{rs}}{1+\varepsilon^{rb}}, \quad . \quad . \quad . \quad (13)$$

in which $\varkappa$ is Neumann's coefficient of magnetization by induction, and is equal to

$$\frac{\mu-1}{4\pi}$$

This equation then gives

$$Q''_\varepsilon = \Delta L \left(\frac{\pi d^2}{4}\right) \mathfrak{H} r(\mu-1) \frac{\varepsilon^{rs} - \varepsilon^{r(b-s)}}{1+\varepsilon^{rb}}. \quad . \quad . \quad (14)$$

Equation (5) can be approximately adapted to this case by making $s' = \infty$, which is equivalent to neglecting those lines of force which pass out of the end section of the bar. This gives $A' = -1$, hence

* I take this occasion to correct an error in Jenkin's "Textbook of Electricity," where it is stated that, by the introduction of the iron bar into the helix, the number of lines of force is increased 32 times. The number should have been from a quite small number for a short thick bar and hard iron to nearly 6000 for a long thin bar and softest iron.

$$Q''_\varepsilon = \Delta L \frac{\mathfrak{H}}{\sqrt{RR'}} \frac{\varepsilon^{r(b-x)} - \varepsilon^{rx}}{1+\varepsilon^{rb}}. \quad . \quad . \quad . \quad (15)$$

Now we have found (equation 7) that $r = \frac{2}{d}\sqrt{\frac{1}{\pi\mu R'}}$ nearly, and this in Green's formula (equation 14) gives

$$Q''_\varepsilon = \Delta L \frac{\mathfrak{H}}{\sqrt{RR'}} \frac{\mu-1}{\mu} \frac{\varepsilon^{rx} - \varepsilon^{r(b-x)}}{1+\varepsilon^{rb}}, \quad . \quad . \quad (16)$$

which is identical with my own when μ is large, as it always is in the case of iron, nickel, or cobalt at ordinary temperatures.

When x is measured from the center of the bar, my equation becomes

$$\lambda = \frac{\mathfrak{H}}{4\pi\sqrt{RR'}} \frac{\varepsilon^{rx} - \varepsilon^{-rx}}{\varepsilon^{\frac{rb}{2}} + \varepsilon^{-\frac{rb}{2}}}. \quad . \quad . \quad . \quad (17)$$

The constant part of Biot's formula is not the same as this; but for any given case it will give the same distribution.

Both Biot and Green have compared their formulæ with Coulomb's experiments, and found them to represent the distribution quite well. Hence it will not be necessary to consider the case of steel magnets very extensively, though I will give a few results for these farther on.

At present let us take the case of electro-magnets.

For observing the effect of the permeability, I took two wires 12·8 inches long and ·19 inch in diameter, one being of ordinary iron and the other of Stub's steel of the same temper as when purchased. These were wound uniformly from end to end with one layer of quite fine wire, making 600 turns in that distance.

In finding λ from Q''_ε, the latter was divided by $4\pi\Delta L$, except at the end, where the end section was included with ΔL in the proper manner. x was measured from the end of the bar in inches.

TABLE XI. Iron Electro-magnet.

x=distance from end.	Q_ε. Observed.	$4\pi\lambda$. Observed.	$4\pi\lambda$. Computed.	Error.
0	22·5	41·1	33·9	−7·2
½	12·6	25·1	26·9	+1·8
1	19·3	19·3	18·9	−0·4
2	12·0	12·0	11·7	− ·3
3	6·6	6·6	7·1	+ ·5
4	3·9	3·9	4·0	+ ·1
5	2·9	2·9	1·7	−1·2
6				

$4\pi\lambda = 42.$

The observations in Table XI. are the mean of four observations made on both ends of the bar and with the current in both directions.

The agreement with the formula in this table is quite good; but we still observe the excess of observation over the formula at the end, as we have done all along. Here, for the first time, we see the error introduced by the method of experiment which I have before referred to in the *apparently* small value of $4\pi\lambda$ at $x = \cdot 75$.

On trying the steel bar, I came across a curious fact which, however, I have since found has been noticed by others. It is that when an iron or steel bar has been magnetized for a long time in one direction and is then demagnetized, it is easier to magnetize it again in the same direction than in the opposite direction. The rod which I used in this experiment had been used as a permanent magnet for about a month, but was demagnetized before use. From this rod five cases of distribution were observed: first, when the bar was used as an electro-magnet with the magnetization in same direction as the original magnetism; second, ditto with magnetization contrary to original magnetism; third, when used as a permanent magnet with magnetism the same as the original magnetism; fourth, ditto with magnetism opposite; and fifth, same as third but curve taken after several days. The permanent magnetism was given by the current.

TABLE XII. Stub's Steel.

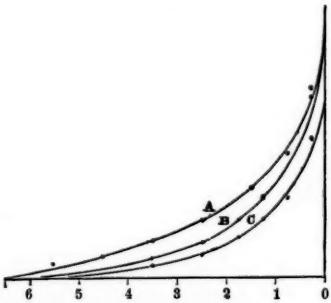
x.	Electro-magnet.				Permanent magnet.					
	Magnetism same as original.		Magnetism opposite to original.		Magnetism same as original.		Magnetism. opposite to original.		Ditto third After three or four days.	
	Q_ϵ.	$44\pi\lambda$.	Q_ϵ.	$4\pi\lambda$.	Q_ϵ.	$\pi\lambda$.	Q_ϵ.	$4\pi\lambda$.	Q_ϵ.	$4\pi\lambda$.
0	23·3	42·5	15·9	29·0						
½	11·5	23·0	7·7	15·4	14·4	13·7	4·8	4·6	12·8	12·2
1	8·2	16·4	5·9	11 8						
1½	6·1	12·2	4·3	8·6	8·2	8·2	4·0	4·0	7·3	7·3
2	7·4	7·4	5·5	5·5	5·3	5·3	2·9	2·9	4·8	4·8
3	3·6	3·6	2·7	2·5	3·0	3·0	1·6	1·6	2·9	2·9
4	1·7	·8	1·0	·5	2·2	1·1	·9	·4	2·0	1·0
6										

The observations in Tables XI and XII. can be compared together, the quantities being expressed in the same unknown arbitrary unit. It is to be noted that the bars in Tables XI. and XII. were subjected to the same magnetizing-force.

First of all, from these tables and figures we notice the change in distribution due to the quality of the substance;

thus in fig. 5 we see that the curves for steel are much more steep than that of iron, and would thus give greater values to r in the formula, a result to be expected. We also observe in both figures the great change in distribution due to the direction of magnetization. In the case of the electro-magnet this amounts to little more than a change in scale; but in the permanent magnet there is a real change of form in the curve. It seems probable that this change of form would be done away with by using a sufficient magnetizing-power or magnetizing by application of permanent magnets; for it is probable that the fall in the curve E is due to the magnetizing-force having been sufficient to change the polarity completely at the center, but only partially at the ends.

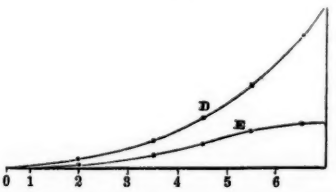
5.



Results from electro-magnets:—
A. Iron, from Table XI.
B. Steel, from Table XII, magnetized same as originally.
C. Steel, from Table XII, magnetized opposite to its original magnetism.

On comparing the distribution on electro-magnets with that on permanent magnets, we perceive that the curve is steeper toward the end in electro-magnets than in permanent magnets. At first I thought it might be due to the direct action of the helix, but on trial found that the latter was almost inappreciable. I do not at present know the explanation of it.

6.



Results from steel permanent magnets:
D. Magnetized in its original direction, Table XII.
E. Magnetized opposite to its original direction, Table XII.
Scale four times that of fig. 5.

As before mentioned, Coulomb has made many experiments on the distribution of magnetism on permanent magnets, and so I shall only consider this subject briefly. I have already given one or two results in Table XII.

The following tables were taken from two exactly similar Stub's steel rods not hardened, one of which was subsequently used in the experiments of Table XII. They were 12·8 inches long and ·19 inch in diameter.

The coincidence of these observations with the formula is very remarkable, but still we see a little tendency in the end observation to rise above the value given by the formula.

TABLE XIII.

x.	Q_ϵ. Observed.	$4\pi\lambda$. Observed.	$4\pi\lambda$. Computed.	Error.
0				
1·28	46·6	34·9	34·26	+·6
2·56	23·8	18·6	18·60	0
3·84	12·6	9·8	9·88	−·1
5·12	7·2	5·6	4·77	+·8
6·40	2·3	1·8	1·41	+·4

$4\pi\lambda = \cdot 117(10^{\cdot 203(b-x)} - 10^{\cdot 203x})$.

In equation (7), and also from Green's formula, we have seen that for a given quality and temper of steel $\frac{rd}{2}$ is a constant. From Coulomb's experiments on a steel bar ·176 inch in diameter whose quality and temper is unknown, though it was

TABLE XIV.

x.	Q_ϵ. Observed.	$4\pi\lambda$. Observed.	$4\pi\lambda$. Computed.	Error.
0				
1·28	42·6	31·9	30·74	+1·2
2·56	21·4	16·7	16·72	0
3·84	10·9	8·5	8·86	− ·4
5·12	5·4	4·2	4·28	− ·1
6·40	1·7	1·33	1·27	+ ·1

$4\pi\lambda = \cdot 105(10^{\cdot 203(b-x)} - \cdot 203x)$.

probably hardened, Green has calculated the value of this constant and obtained ·05482, which was found from the French inch as the unit of length, but which is constant for all systems. From Tables XIII. and XIV. we find the value of r to be ·4674, whence $\frac{rd}{2} = \cdot 04440$ for steel not hardened. As the steel becomes harder, this quantity increases and can probably reach about twice this for *very* hard steel.

TABLE XV.

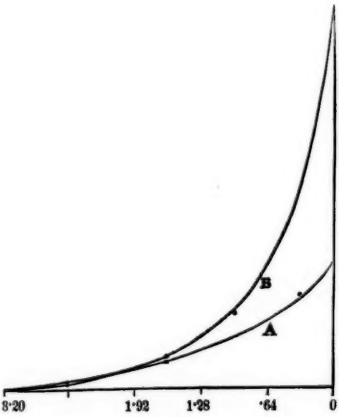
x.	Soft steel, A.		Hard steel, B.	
	Q_ϵ.	$4\pi\lambda$.	Q_ϵ.	$4\pi\lambda$.
0				
·64	20·4	29·1	47·7	68·1
1·28	9·8	15·3	13·9	21·7
1·92	6·0	9·4	7·0	11·0
3·20	3·8	3·0	2·6	2·0

To show the effect of hardening, I broke the bar used in Table XIV. at the center, thus producing two bars 6·4 inches

long. One of these halves was hardened till it could scarcely be scratched by a file, but the other half was left unaltered. The following table gives the distribution, using the same unit as that of Tables XIII. and XIV. The bars were so short that the results can hardly be relied on; but they will at least suffice to show the change.

In fig. 7 I have attempted to give the curve of distribution from Table XV, and have made the curves coincide with observation as nearly as possible, making a small allowance, however, for the errors introduced by the shortness of the bar. It is seen that the effect of hardening in a *bar of these dimensions* is to increase the quantity of magnetism, but especially that near the end. *Had the bar been very long, no increase in the total quantity of magnetism would have taken place, but the distribution would have been changed.* Hence from this we deduce the important fact *that hardening is most useful for short magnets. And it would seem that almost the only use in hardening magnets at all is to concentrate the magnetism and to reduce the weight.* And indeed I have made magnets from iron wire whose magnetization at the central section was just as intense as in a steel wire of the same size; but to all appearances it was less strongly magnetized than the steel because the magnetism was more diffused; and as the magnetism was not distributed so nearly at the end as in the steel, its magnetic moment and time of vibration was less.

7.



Results from permanent magnets.
A. Soft steel.
B. Hard steel.

It is for these reasons that many makers of surveyors' compasses find it unnecessary to harden the needles, seeing that these are long and thin.

We might deduce all these facts from the formulæ on the assumption that r is greater, the harder the iron or steel.

Having thus considered briefly the distribution on electromagnets and steel magnets, and found that the formulæ represent it in a general way, we may now use them for solving a few questions that we desire to know, though only in an approximate manner.

[To be continued.]

ART. III.—*On Recent Researches in Sound;* by WM. B. TAYLOR.

THAT two so eminent physicists as Professor Tyndall in England, and Professor Henry in our own country, should have been for some time past (and almost simultaneously) engaged in investigating the aberrant actions of Sound, with especial reference to securing increased efficiency to the national systems of Fog-signaling, is a noteworthy circumstance, and one of no slight practical importance. In view of the many disastrous marine accidents resulting from fogs on either coast, every thoughtful mind must regard with profound interest a series of researches requiring so much patient labor for the attainment of new and accurate information on the subject, and so high a degree of scientific sagacity and skill for its right interpretation.

As somewhat different explanations have been offered by these two distinguished observers to account for certain abnormal phenomena of sound, a concise statement of the facts and views respectively announced, will interest the general reader. The records of these investigations are, on the one side, the Philosophical Transactions of the Royal Society of London for the year 1874, vol. clxiv, page 183, "On the Atmosphere as a Vehicle of Sound," by John Tyndall, LL.D., F.R.S., a communication read February 12, 1874; and on the other side, the Annual Report of the Light House Board of the United States for the year 1874; the Appendix to which is an account of the operations of the Board relative to Fog-Signals, by Joseph Henry, Chairman of the Light House Board. In addition to these principal sources of information, reference will be made to an interesting communication read before the Royal Society, April 23, 1874, "On the Refraction of Sound," by Professor Osborne Reynolds, and published in the Proceedings of the Royal Society for 1874. The salient points of the observations are selected, and are here arbitrarily designated by bracketed numbers, to facilitate comparisons.

I.

Ten years ago, or in 1865, Professor Henry commenced his investigations on the subject of Sound in connection with fog-signals, at the Light House station near New Haven, Connecticut. Omitting here his careful experiments in regard to the character of the various instruments employed, the principal results then obtained, were the following:

[1.] The reflection of sound was observed to be very imperfect and inexact. A large concave reflector with a smoothly

plastered surface of 64 square feet, produced a sensible increase of effect in the sound, within a distance of 500 yards in front of the signal: beyond this distance, the difference became imperceptible. It appeared that "while feeble sounds at small distances are reflected as rays of light are, waves of powerful sound spread laterally, and even when projected from the mouth of a trumpet, at a great distance tend to embrace the whole circle of the horizon." (L. H. Rep., p. 88.) A trumpet, however, which could be heard six miles in front (in the direction of the axis) was heard only three miles in the rear. (p. 92.)

[2.] "For determining the relative power of the instruments, the use of two vessels had been obtained." The instruments at the light-house station were a large bell, a steam-whistle 6 inches in diameter, a double whistle, "improperly called a steam gong," 12 inches in diameter, the cups being 20 and 14 inches deep, producing the harmonic interval of a tone and its fifth, and a Daboll trumpet operated by a hot air engine. The blow-off sound from the "exhaust" of the air engine was also noted. "The penetrating power of the trumpet was nearly double that of the whistle." (Rep., p. 90.) The order of audible range on the first day was found to be 1st, trumpet, 2nd, exhaust, 3rd, bell, the whistle not being sounded. On the second day, 1st, trumpet and "gong," 2nd, whistle, 3rd, exhaust. In the rear the trumpet was heard no farther than the whistle. On the third day, the order was similar,—1st, trumpet, 2nd, whistle, 3rd, exhaust, 4th, bell. (p. 91.) The opportunity was unfavorable to the observation of these sounds when they were moving directly with the wind.

[3.] Simultaneous observations from two vessels sailing in nearly opposite directions, showed that the sound did not extend against the wind so far as in the direction of the wind; and on subsequent days, results obtained from sounds moving nearly against the wind, and at right-angles to it, indicated that an opposing wind, when light, obstructed sound less than when stronger, and that wind at right-angles to the sound, permitted it to be heard farther. (Rep., p. 92.)

[4.] "During this series of investigations an interesting fact was discovered, namely, a sound moving against the wind, inaudible to the ear on the deck of the schooner, was heard by ascending to the mast-head." (p. 92.) These results were obtained in 1865.

[5.] An experiment subsequently made at Washington during a fog, with a small clock-work alarm bell, indicated that the fog did not absorb sound; though want of the opportunity of a comparative observation prevented the result from being entirely satisfactory. (p. 93.)

In 1867, the principal object of investigation was a compari-

son of different instruments, the character and value of the improvements made in them and especially an examination of a new fog-signal made under the direction of the Board by Mr. Brown, of New York,—the steam siren (p. 194), an inctrument which has since played an important part in fog-signaling. Employing 1st a large Daboll trumpet, 17 feet long, (its steel tongue being 10 inches long), and operated by a hot air engine, 2nd, a siren operated by a tubular steam boiler, and 3rd, a steam whistle, 8 inches in diameter,—an elaborate series of experiments was made as to their penetrating power, as to the most efficient pitch or tone, (p. 95), the effect of varying steam pressure from 20 pounds per square inch to 100 pounds per square inch, (p. 97), the material and shape of the trumpets, &c. (p. 98.)

[6.] During this series of experiments in 1867, attention was called by General Poe, of the Light House Board, to the circumstance that the sound of the paddle-wheels of a steamer some four and a half miles distant from the shore could be distinctly heard by bringing the ears near to the surface of the beach. This fact had previously been noticed on the northern lakes. The desirability of experimenting with large hearing trumpets placed near the surface of the water is suggested by Professor Henry. (p. 98.)

[7.] Experiments on the divergence of acoustic beams, while indicating a considerable reduction of sound toward the rear of the trumpet, showed also very strikingly, the increasing tendency of sound to spread on either side of the axis of the trumpet. (p. 98.) This corresponds with the observations [1] on the employment of sound reflectors.

An important suggestion is made, requiring experimental determination, namely, that condensed air would probably give more efficient results to both the fog-whistle and the siren, than steam. "From hypothetical considerations this would appear to be the case, since the intensity of sound depends on the density of the medium in which it is produced; and as the steam is considerably lighter than air, and as the cavities of all these instruments are largely filled with steam, the intensity of sound would on this account seem to be less." (Rep., p. 99.)

In the absence of Professor Henry in England in 1870, experiments were continued by General Duane, one of the Light House District engineers. These will presently be noticed.

[8.] In 1872 Professor Henry observed from a steamer in the harbor of Portland, Maine, that while approaching an island from which a fog-signal was audible,—at the distance of two or three miles, the sound was lost for nearly a mile, and then slightly regained at nearer approach. This was partly in the rear of the signal; and from its position on the farther side of

the island from the steamer, with a large house and rising ground interposed, Professor Henry infers that the region of inaudibility was covered by an acoustic shadow, encroached upon at a greater distance by the divergence of the rays of sound, which, bending, reached ultimately the surface of the water. (p. 107.) A similar phenomenon was observed in the same year on approaching Whitehead station near the coast of Maine. The fog-signal was heard from the distance of six miles to about three miles, and then lost until within a quarter of a mile. (p. 107.) Again, at little Gull Island, in a vessel receding from the siren signal in the direction of its trumpet axis, the sound was lost at a distance of two miles, and then regained at a distance of four and a half miles. (p. 111.) These last cases are referred by Professor Henry to a flexure of the rays of sound resulting from differences of wind velocity in the upper and lower strata of air.

[9.] In 1872, it was observed that a fog-signal was heard from one station to another, while a simultaneous signal from the latter was inaudible in the opposite direction. On board a steamer approaching Whitehead station (a mile and a half from the coast of Maine), the signal, a steam-whistle, failed to be heard from the distance of about three miles to about a quarter of a mile from the station; while a smaller whistle on the steamer was distinctly heard by the keeper at the station during that time. The wind was slightly transverse to the direction from the steamer to the station, but approximately in that direction. The steamer after stopping at the station, on passing from it almost directly against a light wind, continued to hear the signal with variable distinctness for about fifteen miles. (p. 108.) In September, 1874, the keeper at Block Island, on the coast of Rhode Island, observed according to instructions, the times when the fog-signal from Point Judith at a distance of seventeen miles was audible, and in comparing the times when the Block Island signal (a powerful steam siren) was heard at Point Judith, it appeared that the two sounds had not been heard simultaneously by the two keepers. (p. 112.)

[10.] In August, 1873, at Cape Elizabeth station in Maine, the phenomenon of ocean-echoes was distinctly noticed on board a steamer as it was passing directly outward from the signal; the sound after each whistle being returned from the unobstructed space beyond. (p. 109.) In September, 1874, at Black Rock Island also, shortly after each blast of the trumpet, a prolonged echo from the open ocean was distinctly heard. The echo was observed not to be loudest at the siren-house, but at a point several hundred yards to one side; the wind being in the direction of the primitive sound, and nearly opposite to the direction of the reflected echo. (p. 112.) This was supposed by Professor

Henry to be caused by a reflection of the sound from the crests and slopes of the waves.

[11.] On September 23rd, 1874, three observations were made on board steamers moving in opposite directions about one and a half miles from Sandy Hook, New Jersey. First, before noon with the wind from the west, second, at noon with the wind lulled to a calm, and third, an hour and a half later, with the wind blowing from the east. These observations gave the unexpected result of the sound being heard in each case uniformly farthest from the west, irrespective of the wind. (p. 114.) On the next day, September 24th, the observations were repeated farther out at sea, or six miles from the nearest land. Small balloons, sent off with each observation on the sound, showed that notwithstanding the change of surface wind as before, from morning to afternoon, the upper current of wind was steadily and continuously from the west. (p. 115.) Professor Henry supposes that in the first case "the motion of the air being in the same direction both below and above, but probably more rapid above than below on account of resistance, the upper part of the sound-wave would move more rapidly than the lower, and the wave would be deflected downward, and therefore the sound as usual heard farther with the wind than against it." In the third case with a local sea-breeze in the opposite direction, and the upper current remaining unchanged, "the sound should be heard still farther in the same direction or against the wind at the surface, since in this case the sound-wave being more retarded near the surface, would be tipped over more above, and the sound thus thrown down." (p. 115.) This explanation derived from a communication of Professor Stokes, at the Dublin Meeting of the British Association in 1857, (Rep. of B. A., 1856, p. 22 of Abstracts) would appear to be a very satisfactory solution of the apparent anomaly.

II.

In 1870, General Duane, the engineer in charge of the Light-house District embracing the coast of Maine, New Hampshire, and Massachusetts, was assigned by the Light House Board, (as one "who from his established reputation for ingenuity and practical skill in mechanism, was well qualified for the work,") to make experiments and observations on fog-signals. Accordingly during the year 1871, extensive investigations were made by him at Portland, Maine. Passing over his valuable remarks on the qualities of fog-signals, the following are the principal facts observed by him:

[A.] The extremely variable range of sound. The steam fog-whistles on the coast of Maine could frequently be heard at a distance of twenty miles, and as frequently could not be heard

two miles, with apparently the same state of the atmosphere. (L. H. Rep., p. 100.)

[B.] The signal was often heard at a great distance in one direction, while scarcely audible at a mile in another direction, and this quite irrespective of the wind. (p. 100.)

[C.] Falling snow was observed not to obstruct sound sensibly, as the steam-whistle on Cape Elizabeth can be "distinctly heard in Portland, a distance of nine miles, during a heavy northeast snow-storm, the wind blowing a gale directly from Portland toward the whistle." (p. 100.)

[D.] The signal station frequently "appears to be surrounded by a belt varying in radius from one to one and a half miles, from which the sound appears to be entirely absent." Receding from the signal, its sound may be audible for the distance of a mile, then lost for the distance of a second mile, and then audible again for a much farther distance. This abnormal phenomenon has been observed at various stations, and at one where the signal is on a bare rock in mid-ocean, twenty miles away from land, and with no surrounding objects to affect the sound. (p. 100.)

No observations have been made to show that this occasional sound-chasm is really a "belt" entirely surrounding the signal; a supposition which appears to be antecedently improbable, and one which would require a large number of radiating observations made simultaneously, to establish it. The curious and exceptional fact, however, is confirmed by the observations of Henry [8] made subsequently.

[E.] Confirmatory of Henry [1], General Duane found that a whistle in the focus of a large parabolic reflector, though giving a notably louder sound in front near the reflector, yet at the distance of a few hundred yards, had its beam of sound so spread that the acoustic shadow behind the mirror vanished, and no perceptible difference appeared. A wooden trumpet or square pyramidal box 20 feet long, in a horizontal position with the whistle in the smaller end, gave, however, more successful results, the increase of sound in the open axis being perceptible at the distance of a mile. (Rep., p. 103.) This corresponds also with Henry's observation [7].

[F.] In repetition and explanation of observation [A] General Duane remarks: "It frequently occurs that a signal which under ordinary circumstances would be audible at the distance of fifteen miles, cannot be heard from a vessel at the distance of a single mile. This is probably due to the reflection mentioned by Humboldt." (p. 104.) This great traveller and scientific observer, in his graphic narrative of exploration in the northern part of South America published at the beginning of the century, ascribes the diminished audibility during the day,

of the noise from the cataracts of the Orinoco, at a place on the Atures, to the unequal heating of the air and the reflection and dispersion of the sound from the surfaces of the striæ of differing density.

[G.] It was further noticed by General Duane that "when the sound is thus impeded in the direction of the sea, it has been observed to be much stronger inland;" tending to confirm his idea that the sound in passing from a warmer to a cooler region of air "undergoes reflection at their surface of contact." (p. 104.)

Professor Henry dissents from this opinion that the extinction of powerful sounds is due to unequal density of the atmosphere. Admitting that "a slight degree of obstruction of sounds may be observed" from such a condition, he thinks it "entirely too minute to produce the results noted." (p. 104.) He believes that the "true and sufficient cause" is the difference between the upper and lower currents of air, which tends to bend the sound rays either upward or downward, as suggested by Professor Stokes in 1857. He adds, "In the comments we have made on the Report of General Duane the intention was not in the least to disparage the value of his results which can scarcely be too highly appreciated." (Rep., p. 106.)

[H.] A difficulty occasionally observed with vessels in a fog, is an apparently false direction of the audible signal; which General Duane regards as "due to the *refraction* of sound in passing through media of different density." (p. 104.)

[I.] While thus adopting "the conclusion that these anomalies in the penetration and direction of sound from fog-signals, are to be attributed mainly to the want of uniformity in the surrounding atmosphere," General Duane was also led from observation and experiments to believe "that snow, rain, fog. and the force and direction of the wind, have much less influence than has generally been supposed." (p. 104.) This is in confirmation of his previous observation [C].

III.

Professor Tyndall commenced his investigations on fog-signals on the 19th of May, 1873, "at the instance of and in conjunction with the elder brethren of the Trinity House," as the scientific adviser of the Corporation.

[I.] On May 20, 1873, observations showed the relative penetrating power of different instruments to be variable. At six miles the fog-horn was inaudible, while an eighteen pound gun with three pound charge was heard for ten miles. On many subsequent occasions the horn was found to be superior to the gun. (*Trans. R. S.*, p. 188.) Occasionally the whistles were superior to the trumpet, though not generally so. (p. 189.)

Later experiments in October showed that the pitch of the sound had variable penetration on different days and even at different times on the same day. The siren (an American instrument lent by the United States Lighthouse Board, and put in use October 8, 1873) was generally decidedly triumphant, but not always so. (*Trans.*, pp. 220, 221.)

[2.] The defect of sound in the acoustic shadow of an intervening obstacle (a chalk cliff) was very strikingly manifested. In June the same sharpness of shadow line was observed; and even with the instruments in view, at the distance of a mile, their sound entirely failed near the shadow line at one side. (*Trans.*, p. 190.)

[3.] Although "the wind exerts an acknowledged power over sound" yet, on the 25th of June, "when the range was only six and a half miles, the wind was favorable; on the 26th when the range exceeded nine and a quarter miles, it was opposed to the sound." (p. 194.) On October 11, the sound was observed to be much affected by an adverse wind. It was also noticed on this as well as on subsequent occasions, that "an opposing wind affects the gun-sound far more seriously than that of the siren." With a favoring wind, sounds were heard twice as far as with an adverse wind, even at a point "more deeply immersed in the sound-shadow." (p. 224.)

[4.] July 1, at a distance of five and a quarter miles from a rotating horn it was observed that the sound was sensibly stronger in front than at the rear of the trumpet, the reduction being estimated as seven to ten. (p. 192.)

[5.] July 1, "In a thick haze, the sound reached a distance of twelve and three-quarter miles, while on May 20, in a calm and hazeless atmosphere, the maximum range was only from five to six miles." (p. 193.) And subsequent observations made in London, December 10 and 11, showed that a thick fog offered no sensible obstruction to the passage of sound. (p. 209, 210.)

[6.] On July 3, at 2.15 P. M. "with a calm clear air and smooth sea," at three miles from the signal station "neither horn nor whistle was heard. The guns were again signaled for; five of them were fired in succession, but not one of them was heard." (p. 194, 195.) As a hot sun was pouring its beams on the sea, Professor Tyndall supposed that the copious evaporation resulting, would most probably act very irregularly, producing streams or wreaths of vapor, and thus render the air *flocculent* with these invisible cloudlets, whose surfaces would occasion a large amount of repeated reflection and dispersion of the sound waves. As the sun afterward became clouded at 3.15 P. M., the sounds of the signal were heard at three miles, and very faintly at four and a quarter miles; and later at six miles, and

seven and three-quarter miles. Toward the close of the day the signals were heard at twelve and three-quarter miles. (p. 196, 197.)

[7.] On the same day at one o'clock, the echoes from the direction of the open sea were very distinct at the signal station. "The instruments hidden from view, were on the summit of a cliff 235 feet above us, the sea was smooth and clear of ships, the atmosphere was without a cloud, and there was no object in sight which could possibly produce the observed effect. From the perfectly transparent air, the echoes came, at first with a strength apparently but little less than that of the direct sound, and then dying gradually and continuously away." (p. 198.) These remarkable echoes are supposed by Professor Tyndall to be returned from the invisible surfaces of the vaporous striæ, which thus render the air opaque to the sonorous waves. Subsequently, on the 8th of October, the American siren being just received and set up, its loud echoes were observed to be "far more powerful than those of the horn," and to last eleven seconds, while those of the horn had eight seconds duration. (p. 199.) On the 15th of October, the direction of the echoes was found to correspond with the principal axis of the direct or primitive sound; the direction of the return sound changing with the rotation of the horn. (p. 200.)

[8.] On October 8th rain and hail were found not to obstruct sound. While in the morning (after a thunder storm) from Dover and the South Foreland across the English channel "for a time the optical clearness of the atmosphere was extraordinary, the coast of France, the Grisnez lighthouse, and the Monument and Cathedral of Boulogne being clearly visible in positions from which they were generally quite hidden; the atmosphere at the same time was acoustically opaque;" and the horn was feebly heard at six miles. (p. 205.) But in the afternoon a storm arose, and although the rain was falling heavily all the way between the signal station at Foreland and the point of observation on the steamer, "the sound instead of being deadened, rose perceptibly in power. Hail was now added to the rain, and the shower reached a tropical violence." "In the midst of this furious squall both the horns and the siren were distinctly heard," and as the shower lightened, diminishing the local pattering on the deck, they were heard "at a distance of seven and a half miles distinctly louder than they had been heard through the rainless atmosphere at five miles." (p. 206.) On the 23d of October, a similar experience was noticed on land, and, contrary to the usual impression, snow was also observed to offer no serious obstacle to sound. (p. 207.)

It must be borne in mind that the investigations by Professor Tyndall were concluded before the publication of the United States Lighthouse Report. And it is noticeable that these two series of original observations thus independently made on the opposite sides of the Atlantic, in the main quite strikingly confirm each other.

Tyndall's notice [1] of the inconstant relative range of different instruments corresponds with Henry (2), though indicating a much more marked variability.

Tyndall's notice [2] of the sound shadow, corresponds generally with Henry [7], and Duane [E], but assigns a sharper definition to its limit; probably in consequence of the intervention of a larger obstacle (a cliff), and an observation within a shorter distance.

Tyndall [3] confirms Henry [3] and [11].

Tyndall [4] corresponds with Henry [7] and Duane [E].

Tyndall [5] confirms by a series of careful observations, the opinion of Henry [5] and Duane [I].

Tyndall [6] confirms Duane [A and F], and in like manner adopts and extends the suggestion of Humboldt as to the cause of acoustic opacity. Professor Tyndall's admirable skill in experimental physics enabled him to illustrate and fortify his hypothesis by exhibiting in a popular lecture an apparatus for producing in an elongated box or tunnel, aerial laminæ of unequal density, through which the sound from a small alarm box failed to excite a sensitive flame. That this mottled condition of the air is therefore a true cause of acoustic obstruction is no longer doubtful. To what extent a similar condition of the atmosphere actually prevails, in view of the law of the diffusion of gases, and how far such usual or unusual inequalities of density in the air are capable of entirely dispersing the powerful sound of a steam trumpet or siren, at the distance of a quarter of a mile, are not so positively determined. With a continuous wind any such condition of aerial "flocculence" might be expected to be very speedily dissipated.

This theory, however, fails entirely to explain the interesting observations of Henry [4, 8, and 9]. It is scarcely credible that a local screen of aerial flocculence could obliterate on the deck of a schooner, a fog-signal audible at the mast-head. Atmospheric refraction on the other hand, completely satisfies the observed condition; an opposing wind blowing at the time. Still less successful is the theory, in dealing with the abnormal phenomenon of simultaneous audibility at long range, with the intermediate "belt" of acoustic opacity, first observed by Duane [D]. And lastly, the assumption of simultaneous transmission of sound *through* a flocculent air-screen in one direction and its absorption or dissipation by the screen in the opposite

direction, (acoustic "non-reversibility,") is obviously inadmissible. Nor is the supposition of acoustic "diffraction" around the defined edge of a vapor cloud, more available.

Professor Tyndall in his recent Preface to the last edition of "Sound" remarks upon this observation of Henry [9]—"a sufficient reason for the observed non-reciprocity is to be found in the recorded fact that the wind was blowing against the shore-signal, and in favor of the ship-signal." (*Preface*, p. xxi.) But he offers no suggestion how this "sufficient reason" is supposed to apply. As it is well-known that an ordinary wind cannot increase the range of sound more than two or three per cent (an amount quite inappreciable), this circumstance alone is wholly inadequate to account for the complete suppression of the shore-signal (a ten-inch steam-whistle) from the distance of three miles to *a quarter of a mile*, while the feebler sound of the ship-signal (a six-inch steam-whistle) was making itself distinctly heard throughout the three miles. Something more therefore than the direct or convective action of the wind must be invoked to explain the facts.

Tyndall's observation [7] on the aerial or ocean echoes, corresponds with Henry [10] excepting as to the direction of the principal echo. This difference is doubtless due to the special arrangement of the surfaces or points of reflection in the respective cases observed. Professor Tyndall connects this phenomenon with that of acoustic opacity [6]; and here again his fine experimental skill is brought into requisition to demonstrate the reality of artificial "aerial echoes." By so simple a device as the employment of the flat side of a "bat-wing" gas-jet, the sound beam from a reed instrument was shown to be entirely deflected from one sensitive flame, and reflected back toward another.

This view of a relation between the acoustic opacity outward or seaward, and the reinforcement or reflection of sound inward, is in striking accord with Duane [G], who however in referring to the "reflection' of sound, does not specifically allude to the ocean "echo." On the refraction theory also, a necessary result is that a deflection of the sound-beam upward in one direction, must be attended with a downward deflection and consequent increase of sound in the opposite direction.

Professor Henry had referred these mystic echoes to the crests and slopes of distant waves; (in conjunction probably with a curvature of the sound-beams, constituting a kind of acoustic "mirage.") To this suggestion, Professor Tyndall opposes the observation that "the echoes have often manifested an astonishing strength, when the sea was of glassy smoothness." (*Sound, Pref.*, p. xxiii.)

That this very interesting subject presents features requiring still further and more refined investigation is sufficiently obvious from the single consideration that aerial opacity and echo have not been shown to bear that direct relation which the vapor theory requires. Professor Tyndall has recorded that, on the 17th of October (1873), "It is worth remarking that this was our day of longest echoes, and it was also our day of greatest acoustic transparency, the association suggesting that the duration of the echo is a measure of the atmospheric *depths* from which it comes. On no day, it is to be remembered, was the atmosphere free from invisible acoustic clouds; and on this day when their presence did not prevent the direct sound from reaching to a distance of 15 or 16 nautical miles, they were able to send us echoes of 15 seconds duration." (*Trans.*, p. 202.) If these echoes were not "folded," this would represent an extreme limit of about a mile and a half. Our most powerful sounds cannot afford to waste much of their energy on echoes, if under the inexorable law of increasing attenuation as the square of the distance they are to be audible through a range of 16 miles: less than the 400th of the intensity at one nautical mile, that is heard at the distance of 100 yards from the source; and one 256th of this at the distance of 16 nautical miles, or less than the hundred thousandth of the intensity at 100 yards. And the inference is strong that in such a case accompanying echoes must be derived from sound beams in a somewhat different direction.

Further observations are needed also to ascertain whether these aerial screens of unequal density and acoustic opacity are capable of returning echoes on opposite sides, as is to be expected if we may accept the analogy of catoptrics: and whether the echoes are as frequently heard from steamers in mid-ocean, or whether they mainly attach themselves to coast lines. As Professor Henry has well stated: "Much farther investigation is required to enable us to fully understand the effects of winds on the obstruction of sound, and to determine the measure of the effect of variations of density in the air due to inequality of heat and moisture." (*L. H. Rep.*, p. 117.)

As the last of the series here selected, Tyndall's observation [8] agrees well with the observation of Duane [1].

[To be concluded.]

ART. IV.—*Effect of Temperature on the Power of Solutions of Quinine to rotate Polarized Light. The corrections to be applied for the same. Suggestions regarding the preparation to be used when Quinine is employed as a Medicine;* by JOHN C. DRAPER, Professor of Natural History, College of the City of New York.

IN an admirable article on "The Action of the Solution of certain Substances on Polarized Light," by O. Hesse, in the Annalen der Chemie for 1875, the writer after dealing at length with the varying action of the alkaloids on a beam of polarized light says: "If we now take into consideration the fact that transparent bodies, as water and alcohol, are able, under the influence of electro-magnetism to deflect the plane of polarized light, although this property does not otherwise belong to them; and that the optical powers of a substance can be influenced by mere mechanical means, as Scheibler has proved in certain kinds of glass; we must admit, that '*There is no real relation between the rotating power of a substance and its molecules.*'" He then adds, "The rotating power of a substance is simply the result of the variable action of its factors, viz: the arrangement of the molecules as regards the volume, the solvent, the temperature, the concentration, the chemical combination, the dissociation and other things."

The importance of utilizing the rotation power of quinine for the practical purposes of analysis has induced me to endeavor to determine, as far as possible, the corrections to be applied for the variations in question, and especially for those dependent on temperature. Concerning this, A. Bouchardat says, "variation in temperature causes variation in the rotation power of quinine." In the paper mentioned above, O. Hesse says, "in the case of Thebaine and Quinine the rotation diminishes under an increase of temperature;" but he afterward adds, "I found that the variation between 15° C. and 25° C. was insignificant."

In my experiments the polariscope employed belonged to my friend, Dr. R. A. Witthaus. It was made by Laurent, of Paris, and read by verniers to two minutes. The tube was of glass 220 millimeters in length, with a lateral aperture near the center, through which a thermometer was introduced for the determination of temperature. Around this tube I placed a water jacket, the temperature of which was easily raised to and kept at any required degree, by the injection of steam through a pipe which passed to the bottom of the jacket. Having satisfied myself by a series of experiments that extreme variations of temperature in the water of the jacket, or bath, did not produce

any appreciable effect upon the indications of the instrument itself, I proceeded to the determination of the rotation power of the purest sample of quinine I could procure.

Bearing in mind the statement quoted above, that the concentration, solvent and chemical combination have their influence on the amount of rotation, I assumed specific conditions for the preparation of the experimental solutions which might be easily reproduced. They were, 1st, the use of the uncombined alkaloid quinine, carefully dried over strong sulphuric acid, 2d, ninety-seven per cent alcohol as the solvent and a concentration proportion of one gram of quinine, to fifty cubic centimeters of the alcoholic solution. For the sake of convenience the factors required in calculating the results are presented in the following tabular arrangement, viz:

v. volume of 97 p. c. alcoholic solution=50 cubic centimeters.
p. weight of quinine = 1 gram.
λ. length of tube =220 millimeters.
α=angle of rotation observed with sodium flame.

The formula being $[\alpha]j = \frac{\alpha \times v}{\lambda \times p} \times 100$ and the average of 200 observations on four solutions at a temperature of 25° C. being $\alpha = -6{\cdot}789°$ we have

$$(1) \quad [\alpha]j = \frac{-6{\cdot}789° \times 50}{220 \times 1} \times 100 \text{ or}$$
$$[\alpha]j = -154{\cdot}30° \text{ at } 25° \text{ C.}$$

Raising the temperature to 47° C. the average of 200 observations on the same solutions as before was $\alpha = -6{\cdot}245°$ from which by the formula we have

$$(2) \quad [\alpha]j = \frac{-6{\cdot}245° \times 50}{220 \times 1} \times 100 \text{ or}$$
$$[\alpha]j = -141{\cdot}93° \text{ at } 47° \text{ C.}$$

The difference of temperature in (1) and (2) being 22° C. and the difference in the angle of rotation 12·37°, it follows that 1° C.= ·562° difference.

That is, in a solution of quinine of the strength in question, viz: 20 milligrams of alkaloid to one cubic centimeter of alcoholic solution, for each additional degree Centigrade of temperature the angle of rotation diminishes ·562 of a degree.

To ensure the correctness of these figures I caused my assistant, Mr. Ivan Sickels, also to carry out a series of experiments, and the result of seven hundred observations at temperatures between 25° C. and 47° C. gave figures which only differed in the third decimal place. We are therefore justified in employing the correction in question for values in the vicinity of 25°

C. which closely approaches the temperature at which such observations are made in actual practical work.

Effect of variation in the Proportion of Alcohol in the Solution.—Hesse having shown that the strength of the alcohol has a marked effect on the rotating power of quinine, it followed that perhaps variation in the proportion of quinine dissolved in a given specimen of alcohol would also give variation in the power of rotation. In the examination of this problem I employed a freshly prepared solution of one gram of undried quinine in 50 cubic centimeters of 97 per cent alcohol. The average of 100 readings of the angle of rotation at various temperatures from 20° C. to 50° C. was

$$(3) \quad \alpha = -6{\cdot}05° \text{ at } 35° \text{ C.}$$

To the above 50 cubic centimeter solution 50 cubic centimeters of the same alcohol were added, forming a solution of half the strength of the first solution. The average of 100 readings at similar temperatures was

$$(4) \quad \alpha = -2{\cdot}61° \text{ at } 36° \text{ C.}$$

To this second solution an equal volume of alcohol viz: 100 c. c. was added, giving a solution of one quarter the strength of the first. The average of 100 similar readings was

$$(5) \quad \alpha = -1{\cdot}27° \text{ at } 36° \text{ C.}$$

In the first solution (3) $p=1$ and $v=50$
In the second solution (4) $p=1$ and $v=100$
In the third solution (5) $p=1$ and $v=200$

By the formula $[\alpha]j = \frac{\alpha \times v}{\lambda \times p} \times 100$ we have

For (3) $[\alpha]j = \frac{-6{\cdot}05° \times 50}{220 \times 1} \times 100 = -137{\cdot}50°$ at 35° C.

For (4) $[\alpha]j = \frac{-2{\cdot}61° \times 100}{220 \times 1} \times 100 = -118{\cdot}64°$ at 36° C.

For (5) $[\alpha]j = \frac{-1{\cdot}27° \times 200}{220 \times 1} \times 100 = -115{\cdot}45°$ at 36° C.

From the above experiments we perceive that the effect of a dilution by alcohol of the solution of quinine is to lessen its power of rotation, and as far as the experiments have been conducted this effect is more marked in the first degree of dilution than in the second.

The repetition of these experiments by Dr. R. A. Witthaus and Mr. Sickels, on a similar series of solutions made with the same alcohol and an undried specimen of quinine, gave the following averages of many hundred readings.

In the 1st solution (6) $p=1\cdot\cdot v=50\cdot\cdot\lambda=220\cdot\cdot\alpha=-5{\cdot}58°$ at 29° C.
In the 2d solution (7) $p=1\cdot\cdot v=100\cdot\cdot\lambda=220\cdot\cdot\alpha=-2{\cdot}40°$ at 31° C.
In the 3d solution (8) $p=1\cdot\cdot v=200\cdot\cdot\lambda=220\cdot\cdot\alpha=-1{\cdot}17°$ at 35° C.

By the formula $[\alpha]j=\frac{\alpha\times v}{\lambda\times p}\times 100$ we have

For (6) $[\alpha]j=\frac{-5{\cdot}58°\times 50}{220\times 1}\times 100=-126{\cdot}82°$ at 29° C.

For (7) $[\alpha]j=\frac{-2{\cdot}40°\times 100}{120\times 1}\times 100=-109{\cdot}09°$ at 31° C.

For (8) $[\alpha]j=\frac{-1{\cdot}17°\times 200}{220\times 1}\times 100=-106{\cdot}36°$ at 35° C.

Here again we perceive that the effect of dilution is to diminish the power of rotation, and to about the same extent and in the same manner as in my series of observations. It is therefore evident, that to secure results suitable for a reliable comparison, the solutions of quinine employed should be as nearly as possible of the same strength. The proportion which according to my experience it is most desirable to use is that of about one gram of alkaloid to 50 cubic centimeters of alcoholic solution. While a greater strength than this does not present any advantage in a tube of 220 millimeters, it is objectionable on account of its obstructing the passage of the light.

Quinine combined with Sulphuric Acid.—For the examination of this compound of quinine I prepared a solution which held the same proportion of quinine alkaloid in a given portion of the solution as that contained in the alcoholic solution. The solution was made by taking one gram of dried quinine, dropping it into about 30 cubic centimeters of distilled water, and adding just sufficient sulphuric acid to dissolve it.* The quantity was then made up to 50 centimeters with distilled water, and the 220 millimeter tube filled therewith.

At a temperature of 21° C. the average rotation as determined by 100 observations was $-11{\cdot}36°$. By the formula

$$[\alpha]j=\frac{\alpha\times v}{\lambda\times p}\times 100 \text{ we have}$$

(9) $[\alpha]j=\frac{-11{\cdot}36°\times 50}{220\times 1}\times 100=-258{\cdot}18°$ at 21° C.

The temperature of the solution in the tube was then raised by means of the water jacket, and the average of 100 observations was $\alpha=-10{\cdot}73°$ at 43° C. By the formula

(10) $[\alpha]j=\frac{-10{\cdot}73°\times 50}{220\times 1}\times 100=-243{\cdot}86°$ at 43° C.

* This solution was employed as being similar to that used by physicians.

The difference in temperature being 22° C. and the difference in rotation 14·32°, we have

1° C. = ·650° difference in rotation.

That is, for every rise of one degree Centigrade the rotation diminishes ·650 or nearly two thirds of a degree in a solution of sulphate of quinine in which there is one gram of alkaloid to 50 cubic centimeters of solution.

Effect of variation in the proportion of water.—A solution of sulphate in water prepared as before and containing one gram of alkaloid to 50 c. c. of solution when examined under a variety of temperatures, gave as the average result

(11) $\alpha = -11{\cdot}03°$ at $31\frac{1}{2}°$ C.

This solution diluted by an addition of 50 c. c. of distilled water by which v was raised from 50 to 100 gave under a similar variety of temperatures the average

(12) $\alpha = -5{\cdot}18°$ at 32° C.

Adding 100 c. c. of water to the last solution and thereby raising v to 200, gave under the same conditions

(13) $\alpha = -2{\cdot}58°$ at $31\frac{1}{2}°$ C.

Arranging these in a tabular form we have

For (11) $p=1 \cdot\cdot v=50 \cdot\cdot \alpha=-11{\cdot}03°$
For (12) $p=1 \cdot\cdot v=100 \cdot\cdot \alpha=-5{\cdot}18°$
For (13) $p=1 \cdot\cdot v=200 \cdot\cdot \alpha=-2{\cdot}58°$

From these by the formula $[\alpha]j = \frac{\alpha \times v}{\lambda \times p} \times 100$ we have

For (11) $[\alpha]j = \frac{-11{\cdot}03° \times 50}{220 \times 1} \times 100 = -250{\cdot}70°$ at $31\frac{1}{2}°$ C.

For (12) $[\alpha]j = \frac{-5{\cdot}18° \times 100}{220 \times 1} \times 100 = -235{\cdot}45°$ at 32° C.

For (13) $[\alpha]j = \frac{-2{\cdot}58° \times 200}{220 \times 1} \times 100 = -234{\cdot}54°$ at $31\frac{1}{2}°$ C.

Conclusions.

(*a.*) In the case of the sulphate, as has also been shown by Hesse and others, there is a greatly increased rotation power imparted to the alkaloid by its union with the acid. In the experiments presented the values are: for one gram of alkaloid to 50 cubic centimeters of solution $[\alpha]j = -154{\cdot}30°$ at 25° C. for the alkaloid: for one gram of alkaloid + sulphuric acid to 50 c. c. of solution in water $[\alpha]j = -258{\cdot}18°$ at 21° C., which applying the correction of ·650° for each degree Centigrade becomes $[\alpha]j = -255{\cdot}48°$ at 25° C. for the sulphate.

(*b.*) The aqueous solution of sulphate shows the same changes under the influence of temperature as the alcoholic solution of

the alkaloid, the difference being in the case of the alkaloid 1° C. = −562° and in the case of the sulphate 1° C. = −650°.

(*c.*) In both the sulphate aqueous solution and the alcoholic alkaloid solution, there is the same diminished rotation under dilution, and this occurs chiefly in the first dilution as is shown in the following table:

	Alkaloid Solution.	Sulphate Solution.
1st dilution	$[\alpha]j = -137{\cdot}50°$ at 35° C.	$[\alpha]j = -250{\cdot}70°$ at 31½° C.
2d dilution	$[\alpha]j = -118{\cdot}64°$ at 36° C.	$[\alpha]j = -235{\cdot}45°$ at 32° C.
3d dilution	$[\alpha]j = -115{\cdot}45°$ at 36° C.	$[\alpha]j = -234{\cdot}54°$ at 31½° C.

In closing, I would direct attention to the results indicated in conclusion (*a*), wherein we find that the presence of sulphuric acid has changed the rotation power of a given weight of the alkaloid from −154·30° to −255·48°; and I ask, is it not possible, nay, even probable, that the physiological action of the drug may undergo a similar or perhaps even greater increase? In past times it was the custom to administer quinine in the form of a sulphuric acid solution, and the results were certain and prompt even with minute doses. In recent times, on the contrary, the fancy of patients demands that quinine should be given in pill or some allied form; and though greatly increased doses are used, the practitioner finds it is less certain in its effect. The cause of the difference is doubtless the change in molecular arrangement that produces the marked difference in the action of the alkaloid and sulphate solutions on polarized light; and since the action of the sulphate solution is so much greater than that of the alkaloid solution it is evidently the proper form for the administration of Quinine as a Medicine.

College of the City of New York, Oct. 29, 1875.

ART. V.—*Description of some remains of an Extinct Species of Wolf and an Extinct Species of Deer from the Lead Region of the Upper Mississippi;* by J. A. ALLEN.

THE remains described in the present paper form part of the collection of mammalian fossils made many years since by Professor J. D. Whitney, from the lead-crevices and superficial strata of the lead region of Wisconsin, Iowa, and Illinois, being a part of those enumerated by the late Professor Jeffries Wyman in Whitney's Geological Report of the Lead Region of the Upper Mississippi (pp. 421–423), published in 1862.

The collection originally contained, besides those now described, other remains belonging to the genera *Mastodon*, *Megalonyx* and *Platygonus*, and an extinct species of *Bison*. In

addition to these I find an imperfect radius that seems not to differ at all from that of a young male *Cervus Canadensis*, and a part of another radius that does not differ appreciably from the corresponding part of a radius of *Antilocapra Americana*.

The remains of the fossil deer now described are those mentioned by Professor Wyman, namely a left metatarsus, a humerus and a radius, all more or less imperfect.* Professor Wyman described the humerus as "closely resembling that of the red deer, and of intermediate size between this and the humerus of the caribou." As these cervine remains evidently belonged to a species different from any hitherto described, either extinct or living, I propose for it the name *Cervus Whitneyi*, in honor of their discoverer, Professor J. D. Whitney.

The remains of *Canis* consist of a femur, two tibiæ and a humerus (the latter and one of the tibiæ in perfect condition), and may not have been those mentioned by Professor Wyman, although he enumerates parts corresponding to these; since it seems impossible that he could have described them as not differing in size from corresponding parts of the "gray wolf (*Canis occidentalis* Dekay,—*C. griseus* Sabine)," and as being not distinguishable from them; they in reality indicating a species of nearly twice the size of that animal. The rami and "fragment of a right upper jaw" mentioned by Professor Wyman as belonging to the same species are not now in the collection. This species seems to correspond in size quite nearly with the *Canis dirus* which Leidy described (first under the preoccupied name of *primævus*, and still later under the name of *Indianensis*)† from a portion of an upper jaw found with the remains of *Megalonyx*, *Tapirus*, *Equus* and *Cervus Virginianus* in the banks of the Ohio River near Evansville, Indiana, and also with the *Canis Haydeni* Leidy, described later from the Pliocene sands of the Niobrara River from a fragment of a right ramus. Since of the present species we have only a few of the bones of the limbs, it may be better to give it a provisional name than to refer it to either of the species already described, and await the reception of additional material to show their relationship. I accordingly propose for this species the name *Canis Mississippiensis*. As previously noticed, the remains associated with those now described nearly all belonged to extinct species, and to the fauna immediately preceding the

* Another specimen referred to under the head of *Cervus* by Professor Wyman as "an imperfect humerus of a much smaller animal than the preceding" belongs to the extinct peccary, (*Platygonus compressus*).

† *Canis primævus* LEIDY, Proc. Acad. Nat. Sci. Phila., vii, 200, 1854. Journ., Acad. Nat. Sci. Phila., iii, 167, pl. xvii, figs. 11, 12, 1856. (Name preoccupied).
Canis dirus LEIDY, Proc. Acad. Nat. Sci. Phila., 1858, 21. (Same specimen.)
Canis Indianensis LEIDY, Journ. Acad. Nat. Sci. Phila., vii, 368, 1867. (Same specimen.)

present. The bones, though light and somewhat soft, are still white and in an excellent state of preservation, and, though some are broken, have not suffered much abrasion. The humerus of the wolf shows the marks of the teeth of some small rodent.

Canis Mississippiensis, sp. nov.

The remains of this species, consisting of a perfect right humerus, the distal two-thirds of a right femur, an entire left tibia and the greater portion of a right tibia, indicate a species of nearly if not quite twice the bulk of the existing large wolf of the northern hemisphere (*Canis lupus*), and which had a stature fully one-fifth greater, the difference between them being nearly as great as that between *Canis lupus* and *Canis latrans*. The bones do not differ appreciably in respect to form from those of *Canis lupus*. Their measurements (given in millimeters), in comparison with those of the corresponding bones of a specimen of *Canis lupus* (number 268 of the Museum of Comparative Zoology) from Kansas are as follows:—

Comparative Measurements of Bones of *Canis Mississippiensis* and *Canis lupus*.

	C. Mississippiensis.	C. lupus.
Humerus.—Total length,	223	176
Greatest diameter of proximal end,	55	44
Antero-posterior diameter of head,	41	34
Greatest transverse diameter of distal end,	46	37
Greatest antero-posterior diameter of inner condyle,	36	28
Least circumference of shaft,	62	50
Femur.—Total length,	---	193
Transverse diameter of axis and great trochanter,	---	45
Transverse diameter of condyles,	43	35
Antero-posterior diameter of condyles (inner side),	53	39
Least circumference,	56	44
Length of corresponding parts (distal two-thirds),	155	123
Tibia.—Total length,	244	200
Transverse diameter of head,	47	38
Antero-posterior diameter at most elevated point of the tuberosity,	43	35
Transverse diameter of distal end,	31	24
Least circumference of shaft,	52	43

Cervus Whitneyi, sp. nov.

The remains of this species, consisting of a left humerus, entire except lacking the proximal epiphysis, a left radius, also

lacking the distal end, and a right metatarsal, which has also lost the distal termination, indicate a species of about the same proportions as *Cervus Virginianus*, but much larger, considerably exceeding in size *Cervus macrotis*. The measurements given below indicate the fossil species to have been at least one-seventh larger than *C. macrotis*, and apparently more than one-fifth larger than *C. Virginianus*. A comparison of the bones themselves give a stronger impression of the greatly larger size of the fossil species than do the tabulated measurements. In respect to form, the humeri of the three species do not materially differ, although the condyles in *C. macrotis* have a rather greater relative breadth than in either of the other species. The radius also differs but little in form in the three, but in the fossil species the ulna (it has now been broken away and is lost) was solidly anchylosed with the radius nearly throughout its length, being free only near its distal extremity, whereas in *C. macrotis* it is anchylosed for only its middle portion, being not only free proximally as well as distally, but for quite a space near the proximal end does not even touch the radius, there being an interval of fully two millimeters between them. In *C. Virginianus* the radius and ulna are nearly as fully anchylosed as in the fossil species. The metatarsal bone is similar in form to that of *C. macrotis*, except that it is relatively more compressed laterally in its distal portion, and seems to have been (the distal end is lacking) relatively narrower at its lower articulation. In this respect it corresponds more nearly with the distal portion of the metatarsus of *C. Virginianus*, which is much rounder and relatively more slender than that of *C. macrotis*. The metatarsal of the fossil species differs from that of *C. Virginianus*, however, in having the groove of the posterior surface continued much further distally than in that species. In the following table of comparative measurements the specimens taken are middle-aged males, the *Cervus macrotis* (No. 1781 of the Mus. Comp. Zool.), being from the Medicine Bow Mountains, Wyoming Territory, and the *C. Virginianus* (No. 1733 of the Mus. Comp. Zool.) from Maine.

Comparative Measurements of Bones of *Cervus Whitneyi*, *Cervus macrotis*, and *Cervus Virginianus*.

	C. Whitneyi.	C. macrotis.	C. Virginianus.
Humerus.—Total length,	---	227	220
Length from most prox. part of head to most dist. part of inner condyle,	---	203	200
Breadth of condylar surface,	48	42	38
Antero-posterior breadth of inner condyle,	51	42	42
Least circumference of shaft,	85	76	73

	C. Whitneyi.	C. macrotis.	C. Virginianus.
Radius.—Total length,	---	242	230
Transverse breadth of proximal end,	---	39	37
Transverse breadth of distal end,	41	38	36
Least transverse diameter of shaft,	29	25	24
Least circumference,	80	68	65
Metatarsus.—Total length,	---	273	255
Transverse breadth of proximal end,	33	29	28
Antero-posterior breadth of proximal end,	36	32	30
Transverse breadth of distal end,	---	35	33
Least transverse diameter of shaft,	22	21	18
Least circumference of shaft,	67	66	58
Length of corresponding portions (proximal five-sixths),	273	232	220

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Action of Nitric Acid on Silver and Copper, alone and in presence of Nitrates.*—Acworth has examined at length, in the laboratory of Dr. Frankland, the gases which are evolved by the action of nitric acid on metals, both with and without the presence of nitrates in the solution. The following are his conclusions: (1) cold dilute nitric acid acting on copper evolves nearly pure nitrogen dioxide; (2) in presence of a strong solution of cupric nitrate, this same action gives rise to nearly pure nitrogen monoxide; (3) potassium nitrate has no effect; (4) ammonium nitrate causes the evolution of nitrogen and nitrogen monoxide, mixed with some dioxide; (5) nitric acid, acting on zinc or iron in presence of ammonium nitrate, evolves nearly pure nitrogen; (6) mercury under the same circumstances acts similarly; (7) on silver, the gaseous products are nitrogen and nitrogen dioxide, with scarcely a trace of the monoxide; (8) in presence of ammonium nitrate, silver produces nitrogen chiefly, mixed with a little nitrogen dioxide.—*J. Chem. Soc.*, II, xiii, 828, September, 1875. G. F. B.

2. *On the Condensability of the Gaseous Products of the vistillation of Carbonaceous Shales.*—Distillation of carbonaceous shales at a low temperature, is extensively resorted to, as is well-known, for the production of liquid hydro-carbons for illuminating purposes. The large amount of gas simultaneously produced, and its high illuminating power, suggested to Coleman a series of experiments upon the condensability of these gaseous products. For this purpose a compression-pump was provided, by which the gas was condensed into an iron tube. This tube was

slightly inclined, and at about three-fourths of its length, a reservoir was placed. Beyond this the gas passed through copper tubes which were immersed in a freezing mixture. Upon the main tube was a safety valve which allowed the pressure to be regulated at pleasure; this was maintained at about 140 pounds to the square inch. In the first experiment, 538 liters of gas were passed through the apparatus, in the second 467 liters, and in the third 1274 liters. In both reservoirs, 84 c. c. of liquid was obtained in the first experiment, 77 c. c. in the second, and 195 c. c. in the third. Of the 77 c. c., 54 c. c. of sp. gr. ·690 condensed in the first reservoir (i. e., by pressure alone without cold) and 23 c. c. in the second, of sp. gr. ·650. Of the 195 c. c., 114 c. c. of sp. gr. ·691 condensed at + 16°, and 81 c. c., of sp. gr. ·658, condensed at —18°. As a mean therefore each liter of gas yielded about ·158 c. c. of liquid of sp. gr. ·680; which is equivalent to one gallon for each 1000 feet of gas. After this treatment the gas was found to have lost its illuminating power, giving no more light when burned from a bat wing jet than does a Bunsen burner. From this and other facts, the author concluded that ethylene is absent from shale gas. Common coal gas when subjected to this treatment gave no appreciable quantity of liquid. The shale products, by weight, therefore, which are obtained on distillation, are:—non-luminous combustible gas 20·9 per cent; volatile liquids, sp. gr. ·680 dissolved as vapors in the gas 4·9 per cent; commercial paraffins, sp. gr. ·700–·800, 52·3 per cent; tarry acid or basic bodies 21·9 per cent. The author proposes a method for commercially preparing these light oils from the gas.—*J. Chem. Soc.*, II, xiii, 856, Sept., 1875. G. F. B.

3. *On the Medico-legal determination of Arsenic.*—Having occasion to revise, for purposes of physiological investigation, the methods ordinarily employed for the detection of arsenic in the tissues,* Gautier ascertained that they were seriously deficient in quantitative exactness. He thereupon devised an improved method of separating the arsenic from the organic matter, based upon those of Orfila and Filhol, and a modification of the method of Marsh, by which the arsenic is obtained in a weighable form. The former is as follows:—100 grams of the finely divided animal matter is placed in a porcelain capsule with 30 grams pure nitric acid, and moderately heated. At first the mass liquefies, then it thickens and becomes orange-colored. The capsule is taken from the fire and 5 grams pure sulphuric acid are added. Heat is again applied till white fumes appear. Then 10 or 12 grams of nitric acid is allowed to flow drop by drop on the residue, and it is heated to carbonization. An easily pulverizable mass is thus obtained, which is exhausted with boiling water, filtered, the filtrate reduced with a few drops of hydro-sodium sulphite, and precipitated as usual, by a current of hydrogen sulphide. The arsenous sulphide, transformed into arsenic oxide by nitric acid, is ready for the Marsh apparatus. This consists of a

* See this Journal for December, 1875, page 474.

flask of 180 to 200 c. c. capacity, having two tubulures, and placed in a vessel of cold water. In it are placed 25 grams of pure zinc, on which is poured sulphuric acid diluted with five parts of water. The disengaged gas passes through cotton and then through a tared glass tube heated to redness by charcoal for a length of 20 to 25 cm. The air being expelled, the arsenic, mixed with more dilute sulphuric acid is poured into the apparatus in small portions, an hour being required for the introduction of 5 milligrams of arsenous oxide. The action is kept up for two hours longer, by which time all the arsenic has been carried over. Copper sulphate hinders, platinum chloride facilitates the separation of the arsenic from the solution. After the evolution of gas ceases, the tube containing the annulus of arsenic is weighed again and the amount of arsenic determined. The results are very accurate. In two experiments, in which 5 milligrams arsenous oxide were mixed with 100 grams muscular tissue, the rings weighed 3·72 and 3·67 milligrams respectively; the theoretical quantity being 3·79 milligrams. In a third, 2½ milligrams arsenous oxide were mixed with 100 grams blood; the annulus weighed 1·78 milligrams, the calculated weight being 1·88 milligrams. In 2·1 grams of the brain of a rabbit, fed for 15 days with doses of arsenous oxide gradually increasing from 5 to 50 milligrams, the arsenic recovered was sufficient to give a brilliant ring nearly a centimeter long. A vigorous dog was fed with gradually increasing doses of arsenous oxide, from 4 to 80 milligrams, for a month. 100 grams of the liver yielded 5·3 milligrams, and 100 grams of muscle yielded 0·27 milligram of metallic arsenic.—*Bull. Soc. Ch.*, II, xxiv, 250, Oct., 1875. G. F. B.

4. *Formation of Nitrites by Bacteria.*—The presence of nitrites in spring waters, which has usually been ascribed to the oxidation of ammonia therein, is now stated by MEUSEL to be produced by the reduction of nitrates by the agency of bacteria. In proof of this, he shows: that such water which contained bacteria and nitrates, but neither ammonia nor nitrites, gave, after standing four days, the reactions of nitrous acid; that antiseptics such as salicylic acid, phenol, benzoic acid, alum, and much salt even, prevent or hinder the production of nitrites; that aqueduct-water containing pure nitrates, which alone does not show the production of nitrites even in presence of bacteria, has this change effected upon the addition of glucose, gum, dextrin, cellulose, starch, etc., in the course of from 2 to 14 days; that freshly distilled water, boiled with glucose and niter, shows no nitrites even after standing for weeks, because bacteria are absent: and that putrefying albuminates reduce nitrates to nitrites. The decomposition of cellulose by bacteria in presence of nitrates proves that niter is not only direct food for plants, but that it also performs by its oxygen an important function in the soil. The author believes that these facts have important bearings in agriculture and in medicine.–*Ber. Berl. Chem. Ges.*, viii, 1214, Oct., 1875.

G. F. B.

5. *On the supposed new Hydrocarbon, C_3H_2.*—A short time ago PINNER described* a new hydrocarbon obtained by the action of sodium upon dichlorallylene, to which he assigned the formula C_3H_2. Further investigation has shown him that the formula is more probably C_3H_4 and that the body in question is either allylene itself or an isomer of it. Assuming dichlorallylene to be $C_3H_2Cl_2$, the action of a molecule of sodium upon one molecule of it would be $C_3H_2Cl_2 + Na_2 = (NaCl)_2 + C_3H_2$; but if C_3H_4 is produced, two stages of the reaction are required; $C_3H_2Cl_2 + (Na)_4 = (NaCl)_2 + C_3H_2Na_2$ and $C_3H_2Na_2 + (H_2O)_2 = (NaOH)_2 + C_3H_4$. In the former case the resulting aqueous solution must contain chlorine and sodium in atomic proportions; in the latter, the sodium is double the chlorine. While more alkali than chlorine was always found, it was far from being twice the quantity. To solve the problem, therefore, the author analyzed carefully the tribromide. While C_3HBr_3 requires 13·0 per cent C and 0·4 per cent H, $C_3H_3Br_3$ requires 12·9 per cent C and 1·1 per cent H. In two analyses the carbon was 13·02 and 12·91, and the hydrogen 1·15 and 1·17 per cent. These results, which contradict the former ones, led the author to examine more carefully the composition of dichlorallylene. He finds that instead of its being $C_3H_2Cl_2$ as assumed, it is really $C_3H_4Cl_2$, having 3·6 per cent hydrogen instead of 1·83 which the first formula requires. This fact harmonizes both the above observations and settles the new hydrocarbon as C_3H_4. Hence the product of the action of chlorine upon aldehyde is not crotonyl chloral, but butyl chloral; though from it, however, crotonic acid has been obtained by Sarnow. This problem, Pinner is now occupied in solving.—*Ber. Berl. Chem. Ges.*, viii, 1282, Nov., 1875. G. F. B.

6. *On Aromatic Compounds containing Arsenic.*—MICHAELIS has published a preliminary note upon phenyl-arsenous chloride, $AsCl_2C_6H_5$, which he obtained by the action of arsenous chloride upon mercury-diphenyl, in a sealed tube at 170°. The reaction is given as follows:

$$Hg(C_6H_5)_2 + (AsCl_3)_2 = HgCl_2 + (AsCl_2C_6H_5)_2.$$

It is a heavy, colorless, strongly refractive liquid, slowly decomposed by water. Investigations upon this and other analogous metallic derivatives are in progress.—*Ber. Berl. Chem. Ges.*, viii, 1316, Nov., 1875. G. F. B.

7. *On Diacetone-alcohol.*—HEINTZ, who has been recently investigating the amines derived from acetone by the action of ammonia, has examined diacetonamine, to see whether the reaction with its nitrite would result, as is general with other amines, in the production of an alcohol; the nitrogen of both being eliminated as gas, and hydroxyl taking the place of amidogen. Diacetonamine oxalate was dissolved in three times its weight of hot water, and cooled to 5°. To the liquid kept constantly stirred, potassium nitrite was gradually added, in amount equal to

* Abstract in this Journal, III, x, 293, October, 1875.

nearly twice the weight of the oxalate. After standing several days, the temperature being allowed slowly to reach that of the atmosphere, the liquid was distilled, whereby some mesityl oxide passed over. The residue, freed from an oily layer by a separating funnel, was neutralized with potassium carbonate and agitated with ether. The etherial solution, dried by calcium chloride, left on distilling off the ether, a liquid boiling between 163·5° and 164·5°, having the formula, $C_6H_{12}O_2$. Its vapor density was 4·19. This is diacetone-alcohol, and it has the rational formula

$$CH_3 . CO . CH_2—\underset{CH_3}{\overset{CH_3}{C}}—OH.$$—*Liebig's Annalen*, clxxviii, 342, Oct., 1875. G. F. B.

8. *A new relation between Electricity and Light.*—Mr. JOHN KERR has succeeded in showing that dielectrified media are birefringent. Two holes were drilled in a block of plate glass so as to leave a space of only about a tenth of an inch between their ends. Copper wires covered with rubber and shellac were inserted into these holes, and the current from an induction coil capable of giving a spark of 20 to 25 cms. passed through them. A second passage is opened to the current of variable length through the air, so that when the spark passes the glass is subjected to an electric strain. The light of a lamp is passed through a Nicol prism, then through the glass, and finally through a second Nicol at right angles to the first. Since every plate of glass exerts a depolarizing action a certain amount of light is transmitted. This is cut off by interposing a second piece of the same plate of glass and slightly turning one of the Nicols. If the plane of polarization is inclined 45° to the line along which the electric action is exerted through the glass on closing the primary circuit, light will be visible in about two seconds. It brightens continuously for nearly half a minute, and if the current is broken, will gradually fade away. The light thus restored cannot be extinguished by any rotation of the analyzer. If the plane of polarization is parallel or perpendicular to the lines of electric force no action is obtained. There is as great an effect with a rapid succession of contrary electrizations as with a continued electrization in one direction.

A small square of glass held edgewise in a vice was then introduced in the path of the beam and compressed feebly horizontally. A second plate like a microscope slide was also inserted and bent by the hands until the light introduced by the depolarization of the first plate was extinguished. From this arrangement of the apparatus, it appears that the dielectrization of the plate glass is equivalent optically to a compression of the glass along the lines of electric force. Dielectrified glass acts upon transmitted light as a negative uniaxial crystal with its axis parallel to the lines of force.

Half a dozen other solids were tried, but only two, resin and quartz, gave results worth mentioning. The great difficulty was to get a sufficiently strong superficial insulation, the masses being

too small. A plate of resin was employed of nearly the same size as the glass. Small squares of thin plate glass were placed in optical contact with its two faces and parallel to each other. It gave evidence of irregular strain near the terminals, separated the red and blue rays by a small angle, and was imperfectly transparent. But its chief defect was that it allowed a spark discharge over its surface a length of 7 inches before the distance of the spark terminals much exceeds 2·5 inches. With all these deficiencies, however, it gave a regular and definite effect, and the action was contrary to that of glass.

A plate of quartz cut perpendicular to the axis, 3 mms. thick and 20 long, was employed with a result similar to that of glass.—*Phil. Mag.*, l, 337. E. C. P.

9. *Waves on Mercury.*—M. C. Decharme states that by blowing through a tube touching the surface of mercury, we may produce a sound and circular waves forming a symmetrical network upon the liquid. The smaller the interior diameter of the tube, the higher and weaker is the sound and the shorter the waves. As long as the diameter is less than about half a millimeter, the sound resembles the buzzing of an insect. When the diameter amounts to ·7 or ·8 mms. the sound assumes a clear and distinct musical character. With larger tubes the note becomes loud. There is a great tendency of the sounds to pass to their harmonics, with slight changes of the pressure of the air or of the length of the immersed portion of the tube, especially when the latter has a diameter of 2 to 5 mms. For this reason it is difficult to obtain single sounds, but they are almost always high and unstable harmonies. The best results are attained with tubes 0·8 to 5 mms. in diameter, held vertically so as just to touch the surface, and cut off perpendicular to their axes. The tube is then connected with a large rubber vessel full of air and compressed by a weight which should be greater the smaller the tube.

In general the sounds thus produced depend, as regards their height, quality and intensity, on the diameter, length and nature of the tube, the thickness of its edges, the form of the edges of the orifice, and on the temperature, pressure and nature of the gas; finally and above all, on the capacity of the reservoir of air, or rather on the harmonic ratio of its volume to that of the sounding tube and of the connecting tube. This last condition is so essential that a tube giving good results with one reservoir will not work satisfactorily with another. An important application of this device is to the production and projection of interference waves in an elliptical vessel. The nodes and loops are in this case clearly marked, fixed and symmetrical. The concord of the third, fifth and octave may be similarly projected on a screen with great clearness by employing two sounding tubes.—*Journ. de Phys.*, iv, 207. E. C. P.

10. *Spectrum of the light of the blue Grotto of Capri.*—Dr. H. W. Vogel, on a recent visit to Capri, tested the light of the blue grotto with a spectroscope. As the entrance is only about four

feet in breadth and height, a large part of the light which enters passes through the water. The spectroscope shows that the red is wholly absorbed by the water, and the yellow is so far enfeebled that the D line was scarcely visible. The green, blue and indigo on the other hand shone out brightly, with the F and *b* lines united in a well-marked absorption line.—*Pogg. Ann.*, clvi, 326.

E. C. P.

11. *Action of Magnetism on an Electric Spark.*—M. H. Becquerel has shown that the action of a powerful electromagnet when its current is broken between its poles, is purely mechanical. The spark in this case is accompanied by an explosion, and takes the form of a small flame, which seems to be projected by the action of the magnet as it would be by a current of air. The same effects may be produced by a bellows or even by the mouth. A little explosion is produced, increasing in strength with the force of the air current. It is generally admitted that the sound accompanying the induction spark is due to a sudden expansion of the air and of the volatilized portions of the electrode, followed by a sudden return of the particles to their former position. According to this view the shorter the time of discharge the louder should be the noise.

The substitution of the purely mechanical action of a current of air seems to show that under the influence of the air current as with the magnet, the effect is due to a sudden rupture of the chain of molecules which transmit the electric current of only short duration, forming the induction current. The time of discharge being thus notably diminished the sound accompanying it assumes a remarkable intensity. The idea of making an air-current act in this way is due to M. de Moncel, who has employed it to separate the spark from the aureole.—*Journ. de Phys.*, iv, 206.

E. C. P.

12. *Interference Fringes.*—M. Nodot suggests the substitution of rhombs of Iceland spar for Nicol's prisms in forming the interference fringes of Fizeau and Foucault. The sunlight is admitted through a narrow slit, and focussed on the slit of the spectroscope by a lens. Two rhombs of spar are then interposed, and between them is placed a plate of quartz, cut parallel to the axis, and inclined 45° to the planes of polarization of the spars. Three images of the rhomb are thus formed, the central one of double the brilliancy of the others, since it is formed by the superposition of two images. Allowing each image in turn to fall on the slit of the spectroscope, it will be seen that fringes are present in all three, those of the two outer images being complimentary to the fringes of the central image. Evidently the brilliancy of the effect will be double that usually attained, since a Nicol's prism necessarily cuts off at least half the light.

The experiment may be varied by forming the three images one above the other, and allowing all to fall on the slit of the spectroscope, when a spectrum is formed in which the upper and lower parts are fainter than the central portion, and the fringes of the

former are complimentary to those of the latter. In other words the bright portions of one set correspond to the dark parts of the other.—*Journ. de Phys.*, iv, 209. E. C. P.

13. *The Wind Theory of Oceanic Circulation. Objections examined;* by JAMES CROLL, of H. M. Geological Survey of Scotland. (From the Philosophical Magazine for October, 1875).*—*The fundamental arguments of the advocates of the gravitation theory.*—1. The gravitation theorists base their argument on two principal assumptions which cannot be conceded. First, they maintain that the existence of polar water in the depths of the ocean is consistent with their theory only; and, secondly, they assume as a necessary condition of the wind theory that the understratum of the ocean should consist of warm water. It is a well recognized fact that the ocean beyond the reach of sun heat is occupied with water of a polar temperature; and they therefore point triumphantly to the fact as at once a proof of their position and a conclusive argument against the wind theory. But, on the other side, it will not be difficult to show that the existence of cold water throughout the ocean depths is as much a necessary result of the wind theory as of the gravitation theory, and that there is no relation whatever between the wind theory and warm water in the depths of the sea.

It is supposed that the return *under-currents* from the polar regions are by far too insignificant to be able to maintain at a polar temperature the great depths of the ocean.

Let us examine this objection. It is freely admitted, nay even strenuously maintained by the advocates of the gravitation theory themselves, that the heating-power of the sun does not extend to any great depth below the surface of the ocean; consequently there is nothing whatever to heat this mass of water underneath except the heat coming through the earth's crust; but the amount of heat derived from this source is so trifling that an under-current from the Arctic regions of no great magnitude would be sufficient to keep the mass at an ice-cold temperature.

On a former occasion† I showed that, taking the rate at which internal heat passes through the earth's surface to be that assigned by Sir William Thomson, the total amount received per annum by the North Atlantic, between the equator and tropic of Cancer, including the Caribbean Sea, is equal to only $\frac{1}{89\frac{1}{4}}$ of that conveyed by the Gulf-stream, on the supposition that each pound of water carries 19,300 foot-pounds of heat,—and that consequently an under-current from the polar regions of not more than $\frac{1}{55}$ the volume of the Gulf-stream would suffice to keep the entire mass of water of that area within 1° of what it would be were no heat derived from the crust of the earth, and an under-current of less than $\frac{1}{77}$ that of the Gulf-stream coming from the polar regions would keep the entire North Atlantic from the equator to the arctic circle filled with ice-cold water. A polar under-current half

* Received for this Journal from the author.

† Philosophical Magazine, June, 1874; "Nature," vol. x, p. 52.

the size of the Gulf-stream would be sufficient to keep the entire water of the globe (below the stratum heated by the sun's rays) at an ice-cold temperature. Internal heat would not be sufficient under such circumstances to maintain the mass 1° Fahr. above the temperature it possessed when it left the polar regions.

In short, whatever theory we adopt regarding oceanic circulation, it follows equally as a necessary consequence that the entire mass of the ocean below the stratum heated by the sun's rays must consist of cold water. For if cold water be continually coming from the polar regions, either in the form of under-currents, or in the form of a general underflow as Dr. Carpenter supposes, the entire under portion of the ocean must ultimately become occupied by cold water; for there is no source from which this influx of water can derive heat, save from the earth's crust, which amount is so trifling as to produce no sensible effect.

It is therefore evident that the great mass of cold water occupying the depths of the ocean cannot be urged as an objection to the wind theory.

2. But it is asserted that the impulse of the wind on the surface of the ocean cannot produce and maintain deep under-currents. This is an objection which has been urged by some eminent physicists; but it is based upon a misapprehension of the manner in which, according to the wind theory, under-currents are produced.

It is true, as the objectors maintain, that a wind simply impelling the water forward will not necessarily produce an under-current, since compensation will more readily take place by return surface-currents, as in this case the path of least resistance will generally be at the surface. But when the general surface of one half of an ocean basin is being constantly impelled forward by prevailing winds in a contrary direction to that in which it is being impelled in the other half, compensation cannot possibly take place by means of return surface-currents. For a full discussion of this point I must refer the reader to my work, "Climate and Time," Chap. XIII.

It is, however, needless to advance arguments *à priori* against the possibiliy of such under-currents; for we have actually several well-known examples of such currents, the particulars of which will also be found in the work to which I refer.

3. But supposing it could be shown that the winds cannot directly produce under-currents, it can nevertheless be demonstrated that they can do so indirectly. A vertical circulation filling the deep recesses of the ocean under the equator with polar-cold water, follows as readily and truly from the wind theory as it does from the gravitation theory. It has been shown that the general tendency of the system of the winds is to impel the surface-water of the equatorial regions into the temperate and polar regions as rapidly as it is heated. But such a transference of surface-water must tend to destroy static equilibrium by making the equatorial too light and the temperate and polar columns too heavy, as truly as though the transference had taken place by

means of difference of temperature. The effect must be to produce a constant ascent of the equatorial column and an *inflow* of cold water below equal to the *outflow* above. In short, the wind must produce a system of circulation precisely the same as that supposed to take place by difference of temperature.

By both theories the cause of the vertical motion is the transference of water from the top of the one column to the top of the other. This vertical motion is therefore as much a necessary consequence of the wind theory as it is of the gravitation theory.

II. Geology and Mineralogy.

1. *On the Gravel and Cobble-stone deposits of Virginia and the Middle States;* by Wm. B. Rogers. (Proc. Boston Soc. Nat. Hist., vol. xviii, 1875, 101.—The deposits here described as occurring in the great river valleys and on the adjoining slopes, at Richmond, Va., Washington, D. C., and elsewhere, consist chiefly of layers of water-worn gravel and stones, with ferruginous sands and clays. In most localities the larger pebbles are found in the upper part of the deposit; but in others, as at Alexandria and Richmond, the cobble-stone deposit is overlaid by bedded sands and gravel. Casts of Scolithus occur in some of the pebbles collected at Washington and Richmond. The deposit at Washington covers the entire plain on which the city is built; it averages 75 feet in height above mean tide, but rises on the north to about 100 feet. Thence it spreads over the slopes, covering the grounds of Columbian College, and the higher hill of the Soldiers' Home, over 200 feet above sea level. In the vicinity of the Capitol the stones are often a foot in diameter, and near Georgetown in a recent excavation some are much larger. The facts point to transportation along the valleys, but by streams of much greater width than those now there. The distance transported may be learned from the fact that the nearest Potsdam or Scolithus sandstone to Richmond is 80 miles, and along James River 160 miles; and that from Washington to the western side of the Blue Ridge is 40 miles, and along the Potomac 50 to 60 miles. Prof. Rogers remarks on the origin of these deposits as follows:

"Speculating on the causes by which these deposits have been formed, it may, on the one hand, be imagined that during the Glacial period the icy covering of the north and west prolonged itself in the valleys of the great rivers, as far south as the James, and even the Roanoke River, bringing down to the belt of land now marking the limit of tide water debris from the Appalachian rocks, mingled with materials derived from the intervening region, and that the grinding and sorting action of the waters subsequently obliterated glacial marking, and gave to the whole deposit the distribution and stratification which it now presents; or, on the other hand, it may be conceived that the transporting force of the rivers themselves, swollen and rapid as they must have been in the closing ages of the Glacial period, brought about the same results. But even in this case, it is

highly probable that glacial action had much to do with the original accumulation of the rocky debris on the flanks of the Blue Ridge, and in the Appalachian valleys beyond."

Prof. Rogers further observes that there is a siliceous and argillaceous formation in Virginia, easily confounded with the finer drift beds, which underlies the Tertiary, and is placed by him at the base of the Cretaceous formation or top of the Jurassic; that sections are exposed in a deep railroad cut between Washington and Baltimore, and on the way to Wilmington; that it is seen beneath the Cretaceous green-sand in Maryland, Delaware, and New Jersey, and near Baltimore was found by Prof. Tyson to contain stumps of Cycads. When the Cretaceous and Tertiary are absent these beds are easily confounded with the stratified drift. Its contact with the superficial deposits was well presented in April, 1875, near Washington, at a cut at the extremity of 16th street, at the base of the Columbian College Hill, and on 14th street where it ascends the same hill.

2. *Report of the Geological Survey of North Carolina*, Volume I. *Physical Geography, Resumé, Economical Geology;* by W. C. Kerr. By authority of the General Assembly. 326, 120 pp. 8vo. Raleigh. 1875.—The Geological survey of North Carolina under Prof. Kerr was commenced in 1866. The report now issued was presented to the Legislature in 1870, but not then ordered for publication; and now it appears in 1875 "under a permissive resolution, out of the working fund of the Survey." A second volume, the preface states, will go to press during the year.

The report commences with an introductory chapter on the Physical Geography of the State. In the course of it the fact is brought out that on the *south* side of the rivers in eastern North Carolina there are usually bluffs and high banks, and on the *north*, swamps and low flats; and that this is a feature also of eastern South Carolina. Further, the Miocene shell-beds were found only on the *south* side of these large rivers. "The cause," according to the author, "is doubtless the rotation of the earth co-acting with the river current;" and he cites the law of motion, wrought out with mathematical demonstrations by Prof. Ferrel, that, "in whatever direction a body moves on the surface of the earth, there is a force arising from the earth's rotation which deflects it to the right in the Northern hemisphere, but to the left in the Southern." The chapter treats of the topographical features and climate of the State and contains a long table of altitudes.

On its Geology the author gives only the general outlines. The series of rocks includes those of the Archæan, which are made to cover (1) a large area running northeastward across the State immediately west of its center and extending westward to include the Blue Ridge; also (2) an area in central North Carolina running north-northeast from Raleigh, and some small areas in that vicinity, one of them to the south on the Cape Fear River; and (3) an area west of the Blue Ridge. The first consists largely of syenyte and related hornblendic rocks, which rarely con-

tain mica and are not schistose; the *second*, of light and gray fine-grained gneisses with some ledges of coarse syenyte and masses of titaniferous magnetite or hematite; one bed of hematite on the Cape Fear River being 40 feet thick; the third of gneiss, hornblendic and mica schists, with some syenyte and coarse granite, and a belt of chrysolyte ledges. The coarse granite of the third area affords much mica in large plates, some 20 inches across; and the mining operations for the mica date back to the mound-builders, the granite veins being "honeycombed with the ancient tunnels and shafts, which were located and excavated with more skill and success than the modern workers have yet attained."

No Silurian rocks are recognized, unless on the extreme western border, within what the map colors as Archæan, where, according to Prof. Bradley, the Lower Silurian exists as a continuation of rocks of that era in Tennessee.

The remaining formations described are the Triassic, the Cretaceous, and the Tertiary. The two Triassic areas, one on Deep River and the other on Dan River, at a distance 75 to 100 miles from one another, have the rocks dipping in opposite directions, those of Deep River, or the more eastern area, dipping southeastward about 15° to 35°, and those of the other northwestward 35°. Prof. Kerr makes the supposition—improbable as it appears to the writer—that the two are the margins of a single anticlinal that once spanned the broad interval between them. The Connecticut River Triassic has in general a similar southeastward dip; but there is no where the opposite side of the anticlinal unless we look to the New Jersey area for it, which is quite too far south to answer.

A large part of the volume is occupied with a chapter on Economical Products of North Carolina. It embraces a large amount of information on soils; marls and fertilizers; peat and muck; ores and mines (among which those of iron are very extensive, and those of gold and copper, also, have been profitably worked); coal (Triassic); graphite, and kaolin; fire-clay, pyrophyllite, corundum, mica, and mineral waters.

The coal field of Deep River has an area of about 300 square miles. Dr. Genth obtained in an analysis of two samples, fixed carbon 63·28, 70·48, volatile matter 25·74, 21·90, ash 10·14, 6·46, moisture 0·84, 1·16=100. It contained sulphur 1·35, 1·02. The Dan River coal afforded the same chemist 75·96 and 76·56 p. c. of fixed carbon, 11·44, 13·56 of ash, the volatile matter in each about 12 per cent.

The report contains also a well-colored geological map of North Carolina and several plates of fossils.

3. *Second Geological Survey of Pennsylvania.*—The following Reports for 1874 have recently been published by the State Board of Commissioners at Harrisburg.

Report of Progress on the Brown Hematite (Limonite) Ore Ranges of Lehigh County, with a description of the mines lying between Emaus, Alburtis and Fogelsville; by FREDERICK PRIME, Jr., Assist. Geol. 74 pp. 8vo, with cuts and maps.

Report of Progress in the Venango Co. District; by JOHN F. CARLL. *Observations on the Geology around Warren;* by F. A. RANDALL. *Note on the Comparative Geology of Northwestern Ohio and Pennsylvania and Western New York;* by J. P. LESLEY. 128 pp. 8vo, with wood-cuts and maps.

Report of Progress in the Laboratory of the Survey, at Harrisburg; by ANDREW S. M'CREATH. 106 pp. 8vo.

Prof. Prime gives in his report a brief account of the topography and geology of the district under examination, and of the rocks with which the limonite ores are associated. These rocks are stated to be of the magnesian limestone series—that is, Lower Silurian beds of the age of the Calciferous sandrock, and the Chazy, Birdseye and Black River limestones. Mr. Prime says: "The great mass of this formation is dolomite; but there occur one and possibly more beds of hydro-mica (or damourite) slate intercalated in it. The limonite ore always accompanies the hydro-mica slate, and with it there is often clay from the decomposition of the slate. The following are three out of five analyses of this slate from Lehigh Co.:

	Si	Al	Fe	Mg	Ca	Na	K	H	
1. Fogelsville	49·92	34·06	0·91	1·77	0·11	0·74	6·94	6·52	= 100·97
2. Hensingerville	45·40	24·69	5·06	13·56	tr.	0·27	5·85	4·80	= 99·63
3. Near Allentown	59·30	30·30		trace	tr.	1·51	6·24	4·70	= 102·05

From the amount of the potash in the first analysis (by Dr. Genth), Mr. Prime calculates the amount of damourite present (with free silica, etc.) to be 55·40 p. c.; in the second (by Mr. S. Castle), 49·70 p. c.; in the third (by Mr. P. G. Salom), 53·02 p. c. This hydro-mica slate, with often associated limonite beds (as a result of alteration), extends, as Mr. Prime observes, from Vermont to Alabama, showing thus a long range of Lower Silurian rocks in the eastern mountain region of North America.*

The report describes further the mines and ores, and gives many analyses. It is followed by a note from the State Geologist, making some explanations with regard to the equivalency of the Pennsylvania formations, and remarking on some topographical changes the country has undergone. Mr. Lesley goes outside of his field in his closing remarks, and states—what is sustained as yet by no adequate stratigraphical evidence—that the "Green Mountain system of Vermont" and "the White Mountain system of New Hampshire," are, like "the Laurentian Mountains of Canada," older than the Potsdam; and that the Green Mountain system, one of these "three great mountain systems of the north," is Huronian. The observations by Mr. Prime in Pennsylvania, above-mentioned, and the parallel facts in the Green Mountain system to which he draws attention, all point as regards the Green Mountains in the opposite direction. The writer has studied stratigraphically the Green Mountain region from Con-

* The first determination of the fact that the so-called talcose slates are mostly hydro-mica slates is accredited to Prof. Dana. The latter gives the credit to others in this Journal (III, iv, 366, 1872), where he named the rock *hydro-mica slate.*

necticut to Vermont, and has found that the hydro-mica and chloritic hydro-mica slates associated with the limonite beds of Berkshire are of the same formation with the hydro-mica, chloritic, and micaceous slates of Graylock and the Taconic range; and with the hydro-mica slates of the ridge lying northeast of Rutland in Vermont, and of others west and north of Rutland; and with the staurolitic schists of the limonite region of Salisbury, Connecticut. Since the limestones associated with the slates of West Rutland abound in distinct Lower Silurian fossils, referred to the Chazy by Billings,* part of the Green Mountain slates and schists are unquestionably Lower Silurian. What is the age of the rest is not yet positively known.

Mr. Carll's report contains the results of his observations in the oil district of Venango County—giving in detail the geological and topographical distribution of the oil, and its distribution also in depth.

Mr. Lesley's notes on the comparative geology of the adjoining parts of the States of Ohio, New York and Pennsylvania, are of great interest. The equivalency and distribution of the formations are discussed, and the age of the oil-bearing beds, and some new views and facts are brought out. Mr. Lesley states that the Catskill sandstone does not thin out westward in New York, as heretofore described, but that it continues into Ohio, and that it includes the "Rock-City" conglomerate of Chatauqua Co., N. Y.; also, that the conglomerate under the Coal measures, to which the Rock-City conglomerate has been referred, "seems nowhere to reach the New York State line, even in outlying patches."

The Chemical Report of Mr. M'Creath contains descriptions and proximate analyses of bituminous coals of many localities, and analyses of various iron ores, limestones and fire clays. It states that the average amount of water in the bituminous coals analyzed is only 1·03 per cent; the average of ash about 5·38 per cent; of phosphorus, ·014 per cent; of volatile combustible matter from bituminous coals of Clearfield Co., 23·64 per cent, of Centre Co., 23·81, of Jefferson Co., 32·60, of Armstrong Co., 34·99 per cent; of fixed carbon in coals of Clearfield and Centre Cos., 68·97; of sulphur in 34 coals from Clearfield Co., 1·36 per cent.

4. *The Vertebrata of the Cretaceous Formations of the West;* by E. D. Cope. 304 pp. 4to, with 57 plates. Report of the U. S. Geological Survey of the Territories by F. V. Hayden, U. S. Geologist in charge, and under the authority of the Department of the Interior. Vol. II. Washington, 1875.—Professor Cope's Report is another of the great works on science due to researches

* These West Rutland fossils are not rare and inconspicuous. Guided by the Rev. A. Wing to the localities he had discovered, I found the rock over the *central* part of the valley in some places full of them, and other specimens less distinct occur in the western margin of the valley; Mr. Wing has observed them also on the eastern margin. The famous marble quarries of the valley are in the intermediate portion of the limestone formation where the metamorphism was more complete. J. D. D.

connected with the Geological Survey of the Territories under the charge of Dr. Hayden. The volume takes up only the Vertebrates of the Cretaceous, leaving those of the Tertiary for a later Report. Each subject, owing to the great number of species discovered in the beds, is a very large one; and the latter will make, as the Preface states, two such volumes. These two are in addition to the quarto volume, in the same series, by Dr. Leidy. The work aims to mention all the species thus far discovered in the Cretaceous west of the Mississippi, and to describe in full those made known by the author. Professor Cope, in an introductory chapter presents his views "on the significance of paleontological science;" then treats in Part I of the classification and distribution of the Cretaceous deposits of the West; in Part II, gives "Descriptions of the Cretaceous Vertebrates of the West;" and in Part III, introduces a Synopsis of the known Cretaceous Vertebrates of North America. The 57 plates of illustrations are full of figures, and well engraved.

The whole number of species of Reptiles in the American Cretaceous beds is stated as follows: Dinosaurs, 18; Pterosaurs, 4; Crocodilians, 14; Sauropterygia (Plesiosaurs, etc.), 13; Testudinates, 48; Pythonomorphs (the Mosasaur tribe), 50 = 147. Of the last tribe, 15 species occur in the Greensand of New Jersey, 7 in the Rotten limestone of Alabama, 26 in Kansas, and one from each, North Carolina, Mississippi and Nebraska. Only four are known from Europe.

Professor Cope, besides bringing out his own large contributions to the subject, mentions also those of Dr. Leidy and Professor Marsh, yet not always in a way to do them full justice. He appears to have forgotten one of his statements, when penning the note to page 124, on *Clidastes propython*. The note reads as follows:

"Professor Marsh (American Journal of Science and Arts, 1872, p. 454,) quotes me as assigning ten cervical vertebræ with articulated hypapophyses to this species. This I have not done, but state (Synopsis of the Extinct Batrachia and Reptiles of North America, p. 221,) that it possesses six such vertebræ. Professor Marsh's statement, and consequent supposition that he first determined the number of cervical vertebræ in the genus Clidastes, are the result of a misapprehension."

But Professor Marsh sustains his remark (this Jour., ii, p. 454, 1872) by reference to page 218 of Cope's Synopsis, where Professor Cope, in drawing out the characters of the genus "Clidastes" from "especially the nearly complete skeleton of *Cl. propython*," says: "Hypapophyses exist on the ten cervical vertebræ," thus recognizing *ten* as the number. And on page 221 of Cope's Synopsis (the page he refers to) there is nothing that sets this aside. Professor Cope, in view, as he says, of the fact that "a considerable number of the vertebræ [in *Cl. propython*] has been lost," gives on that page the following enumeration of the [*by him*] *known* vertebræ: Atlas and axis, 2; cervicals, 6; dorsals, 15;

and next adds, under the line *dorsals*, 15, "at least to be added to this series, 10," making the sum "33." No correction of the previous statement as to the total number of cervical vertebræ is made or suggested.

In the catalogue of works and memoirs on American Cretaceous Reptiles, headed LITERATURE OF THE SUBJECT (pp. 51, 52, 53), Professor Cope has omitted to mention several highly important contributions by others: for example, Dr. Leidy's memoir of 1865, *on Cretaceous Reptiles of the United States*, a quarto of 135 pages, illustrated by 20 plates; a paper by the same author, *on Elasmosaurus* (Proceedings of the Acad. Nat. Sci. Philad., 1870, p. 9); another *on Discosaurus and its allies* (ibid., p. 18), and another *on Hadrosaurus and its allies* (ibid., p. 67); also, the following of Professor Marsh's papers: *Notice of some New Mosasauroid Reptiles* (this Journ., II, xlviii, 392, 1869); *on a New Species of Hadrosaurus* (ibid., III, iii, 301, 1872), and *Note on Rhinosaurus* (ibid., III, iv, 147, 1872). Again, in his *Synopsis of the known Cretaceous Vertebrata of North America*, constituting Part III of the volume, several Cretaceous Reptiles described by others are omitted, and also the following four species of Cretaceous birds described by Professor Marsh: *Graculavus velox*, of New Jersey (this Journal, III, iii. p. 353, 1872); *G. pumilus*, of New Jersey (ibid., p. 364); *G. agilis*, of Kansas (ibid., v, 1873), and *Palæotringa vagans*, of New Jersey (ibid., iii, 366). J. D. D.

5. *Description of new Species of Fossil Plants from Alleghany Co., Virginia; with remarks on the rocks seen along the Chesapeake and Ohio Railroad, near the White Sulphur Springs, Greenbrier Co., West Virginia;* by F. B. MEEK. 19 pp. 8vo. Proceedings of the Washington Philosophical Society. Read before the Society, June 15, 1872. (Received Dec. 8, 1875).—The fossil plants described by Mr. Meek are from Lewis's Tunnell, and occur in the lower part of the Subcarboniferous, near its junction with the upper Devonian. The species are *Lepidodendron scobiniforme* M., *Cyclopteris Lescuriana* M., *C. Virginiana* M., *C. Alleghaniensis* M., besides an undetermined *Stigmaria* and some doubtful *Carpolithes*.

6. *Coal plants of Tinkiako in Southern Shensi in China.*—AD. BRONGNIART has determined the following plants from the southern part of Shensi, one of the western provinces of China (Bull. Geol. Soc. France, 408, 1874): *Pecopteris Whitbyensis*, two species of *Sphenopters*, a leaf of a *Zamia* near *Zamites distans*, fragments of *Lycopodites Williamsoni*, a species of *Palissya*, and also *Bayera dichotoma* Fr. Braun. The species are nearest to the Jurassic plants of Whitby, and not Carboniferous. Subcarboniferous fossils are described from the same region by M.M. Paul Fischer and Bayan (ibid., p. 409 and pl. 16), who report, from shales, the following: *Spirifer lineatus* Mart sp., *Athyris ambigua*, *Meekella Garnieri* (n. sp.), *Productus Davidi* (n. sp.), P. *costatus* Sow., var *cœlestis*, and *Bellerophon tangentialis* Phill.

In this connection, it is to be noted that the coal plants of Chaitung, west of Pekin, obtained by R. Pumpelly, and described by Dr. Newberry (Geol. Res. in China, etc., Smithson. Contrib., No. 202, 1867,) are referred by the latter to the Trias; they embraced the species *Pterozamites Sinensis* Newb., *Podozamites lanceolatus* Lindl., *Pod. Emmonsii* Newb., (*P. lanceolatus* of N. Carolina, Emmons), *Sphenopteris orientalis* Newb., *Pecopteris Whitbyensis* Brongn., *Hymenophyllites tenellus* Newb., *Taxites spatulatus* Newb. Von Richthofen, on the contrary, concluded, from the conformability of the coal-bearing beds to others below containing Paleozoic fossils, that the coal of China (this Journal, II, i, 410, 1870), was for the most part Carboniferous.

7. *The Dawn of Life, being the History of the oldest known Fossil Remains, and their relations to Geological Time and to the development of the Animal Kingdom;* by J. W. Dawson, LL.D., F.R.S., etc. 240 pp. 12mo, with plates and wood-cuts. London, 1875. (Hodder and Stoughton.)—This volume contains a complete account of the history of the discovery of the Eozoon, and of its structure and nature as developed by Dr. Dawson, Prof. Carpenter, and others. The facts described are illustrated by excellent figures; and one of them, facing page 35, representing an Eozoon mass, looks exceedingly like a form of coral—the Stromatoporæ—to which group it was referred by Logan before its interior structure was studied. The subject discussed is of profound geological importance, since it bears on the question as to the first expression of the animal idea in an organism, and the volume is therefore one of great interest. The Eozoon is referred by the author to the section of the Rhizopods containing the Foraminifers, and to the division of the Foraminifers called *Perforata*, to which the *Nummulinidæ*, *Globigerinidæ* and *Lagenidæ* belong, which have calcareous skeletons penetrated by pores. An inferior division, called the *Imperforata*, have calcareous membranous or arenaceous skeletons without pores.

8. *Geographical and Geological Surveys*; by J. D. Whitney. 96 pp. 8vo. From the North American Review for July and October, 1875. Cambridge, 1875. (Welch, Bigelow & Co.)—Professor Whitney in these papers brings to bear the results of his wide experience as a geographical and geological explorer, in a discussion of the objects, methods, and purposes of such surveys, and gives some account of their history in this and other countries. Much information is presented on the topographical maps issued by foreign governments, and on those in progress and needed at home. The history of geological exploration in the United States is treated with considerable detail and with discrimination. The volume is one to which all may go for information and judicious advice as to the ends accomplished by State surveys, and the means required to secure from them the greatest good to the people.

9. *Descriptive Catalogue of the specimens in the Museum of Melbourne, illustrating the rock system of Victoria;* by G. H. F. Ulrich, M.E., F.G.S. 108 pp. 8vo. Melbourne, 1875.—Besides

the detailed description of the individual specimens in the museum, this little book contains some remarks upon the different kinds of rocks of Victoria and their relations, which give it something more than a local interest. A considerable number of analyses are given, especially for the igneous rocks.

10. *Geology of Illinois.*—The last volume of the series of reports connected with the Geological survey of Illinois under Mr. Worthen is now in the binder's hands.

11. *Geological map of the United States and Canada.*—In a few days, a Geological chart of the United States (east of the 104th meridian) and Canada, by Prof. F. H. BRADLEY, will be on sale by Messrs. Ivison, Blakeman, Taylor & Co., 138 and 140 Grand street, New York, the publishers of Professor Dana's works on Geology. Its size is 24 inches by 16. It contains all the detail and accuracy possible on a chart of this size in the present state of the science. The geological areas are well distinguished by a judicious system of lining, instead of by colors, and hence the chart will be afforded, as we understand, at the low rate of one dollar each. It should be in the hands of all students in geology, and is absolutely indispensable to every teacher.

12. *Einleitung in die Krystallberechnung;* von Prof. CARL KLEIN, Erste Abtheilung. 208 pp. 8vo. Stuttgart, 1875.—Prof. Klein develops the subject of Crystallography from the standpoint of Quenstedt, according to whom the relations of the planes of a crystal are shown by the lines in which they are projected upon a given plane of projection. He does not confine himself to the somewhat unwieldy formulas of Quenstedt, however, but makes all calculations of axial relations and parameters by means of spherical triangles in the manner employed by von Kobell. The author has worked out with much care and completeness the solutions of the various problems which arise, and accompanies each with an example performed in full, so that the student cannot fail to comprehend the method. E. S. D.

13. *On Troilite;* by Dr. J. LAWRENCE SMITH.—A paper on troilite by Dr. Smith was read before the French Academy of Sciences on the 22d of November last. The author shows by new and careful analyses that troilite, or the sulphide of iron of meteorites, has the composition he has before obtained for it, represented by the formula FeS, and not that of pyrrhotite (Fe^7S^8) to which species it is referred by M. Saint Meunier in a communication to the Academy of March, 1874. He observes that the specific gravity of troilite, 4·813, also separates it from pyrrhotite, a selected specimen of the latter affording him only 4·642. As this meteoric sulphide is found imbedded in a mass of iron, "the natural supposition is that the sulphur would be saturated with the iron." Hence, he adds, "troilite like schreibersite, is exclusively a celestial mineral.

Rectification of the Geological map of Michigan, embracing observations on the Drift of the State; by Alexander Winchell. 17 pp. 8vo. Salem, 1875. From Proc. Amer. Assoc. for 1875.

III. Botany and Zoology.

1. Karl Koch, *Vorlesungen über Dendrologie. Lectures on Dendrology*, delivered in Berlin in the winter semester of 1874–75. Stuttgart, F. Enke, 1875, pp. 408, 8vo.—These are the notes of a course of popular lectures by Prof. Koch, on a subject in which he is thoroughly at home. They must have been delightful to hear, as they are pleasant to read, and are full of interesting matter. It is only the first course of a series which is to be continued this winter. The first division, of seven lectures, is a history of landscape gardens and gardening. The second division, of as many more, treats of the structure, growth, and life of trees, of the influence of woods upon mankind, and as regulators of temperature and atmospherical changes. The third division, in four lectures, treats of Coniferous trees,—all in a popular way. Prof. Koch insists that the two willows confounded as forms of the Weeping Willow, are neither of them Persian or Assyrian, except by immigration, but natives of a farther east, i. e., of China and Japan. One of them may have reached Western Asia, however, early enough to have been collected by Tournefort, and so to excuse the error fixed by Linnæus by his name of *S. Babylonica*. But even the last volume of DeCandolle's Prodromus does not rectify it. Notwithstanding Prof. Koch's correction and elucidation, it is likely that popular books and the popular belief will continue to associate the Weeping Willow with the River of Babylon and the hanging harps of the weeping Israelites, although the tree of the Psalm most likely was a Poplar. We believe it was Ker Porter who remarked that willows were to be found along the river, but only as low shrubs: upon these nothing larger than a comparatively modern musical instrument associated with the name and nation could well be hung. When the course is completed we shall look for an English edition of these lectures upon tree-lore. A. G.

2. *Insectivorous Plants;* by Charles Darwin. With illustrations. London: Murray. New York: D. Appleton & Co.—This long expected work appeared last autumn, was immediately reprinted by the American publishers, and before this time has been so widely read that no detailed account of it is at all necessary. Its main topic is *Drosera* or Sundew, upon which the vast number and diversity of the observations and experiments—at once simple, sagacious, and telling—which it records, are about as wonderful as the results. As to the latter, it is established beyond question that the common Sundews are efficient fly-catchers; that the stalked glands, or tentacles, as Mr. Darwin terms them, are sensitive and turn inward or even in other required directions in response to irritation; that they equally respond and move in obedience to a stimulus propagated from a distance through other tentacles and across the whole width of the leaf; that the sensitiveness belongs only to the glands and tips of the tentacles, but

is propagated thence down their stalks and across the blade of the leaf through the cellular tissues; that they accurately and delicately discriminate animal or other nitrogenous matter from anything else; that the glands absorb such matter; that when excited by contact, or by the absorption of nitrogenous matter by the viscid enveloping liquid, an acid secretion is poured out and a ferment analogous to pepsin, the two together dissolving animal matter; so that the office and action of these glands are truly analogous to those of the glands of the stomach of animals. Finally that animal or nitrogenous matter, thus absorbed and digested in the glands, is taken in, and conveyed from cell to cell through the tentacles into the body of the leaf, was made evident by ocular inspection of the singular changes in the protoplasm they contain. So particularly have the investigations been made and so conscientiously recorded, that the account of those relating to one species of Sundew, *Drosera rotundifolia*, fills 277 pages of the English edition, or more than half of the book. After all it ends with the remark: "and we see how little has been made out in comparison with what remains unexplained and unknown." The briefer examination of six other Sundews follows, some of them equally and others less efficient fly-catchers and feeders.

Dionæa is next treated, but with less detail. Indeed, except as to the particular nature of the secreted digesting fluid, there is little in this chapter that had not been made out or already become familiar here. That the secretion has digestive powers, and that it is re-absorbed, along with whatever has been digested, is now proved beyond reasonable doubt. That the motor impulse is conveyed through the cellular parenchyma, and not through the vascular bundles, or spiral vessels, and that the latter do not originate the secretion, as Rees and Wills in a recent paper seem to suppose they must, appears to be shown by the facts, and was antecedently probable. "The wonderful discovery made by Dr. Burdon Sanderson is now universally known: namely, that there exists a normal electrical current in the blade and footstalk, and that when the leaves are irritated the current is disturbed in the same manner as takes place during the contraction of the muscle of an animal." The conclusion here needs to be checked by parallel experiments, to see whether the same reversion of current does not take place whenever a part of any leaf or green shoot is forcibly bent upon itself.

Aldrovanda vesiculosa, of the *Drosera* family, "may be called a miniature aquatic *Dionæa*;" for, as discovered by Stein in 1873, "the bilobed leaves open under a sufficiently high temperature, and when touched suddenly close." Being submerged, their prey is confined to minute aquatic animals. For want of proper material and opportunity, Mr. Darwin was able to follow up only for a little way the observations of Stein and Cohn,—enough, however, to show that it also captures and consumes animals, but perhaps avails itself of the nitrogenous matter only when passing into decay.

Drosophyllum, a rare representative of the order, confined to Portugal and Morocco, grows on the sides of dry hills near Oporto; so that, as to station, it is the very counterpart of *Aldrovanda*. Its leaves are long and slender, in the manner of our *Drosera filiformis*, and are covered with much larger glands. To these flies adhere in vast numbers. "The latter fact is well known to the villagers, who call the plant the 'fly catcher,' and hang it up in their cottages for this purpose." Mr. Darwin found the glands incapable of movement, and their behavior in some other respects differs from that of *Drosera*; but they equally secrete a digestive juice. Insects usually drag off this secretion instead of being fixed on the glands by it; but their fate is no better; for as the poor animal crawls on and these viscid drops bedaub it on all sides, it sinks down at length exhausted or dead, and rests on a still more numerous set of small sessile glands which thickly cover the whole surface of the leaf. These were till then dry and inert, but as soon as animal matter thus comes in contact with them, they also secrete a digestive juice, which, as Mr. Darwin demonstrated, has the power of dissolving bits of coagulated albumen, cartilage, or meat, with even greater readiness than that of *Drosera*.

Mr. Darwin next records various observations and experiments upon more ordinary glandular hairs of several plants. To certain Saxifrages his attention was naturally called, on account of the presumed relationship of *Droseraceæ* to this genus. He declares that "their glands absorb matter from an infusion of raw meat, from solutions of nitrate and carbonate of ammonia, and apparently from decayed insects. To such plants the vast number of little insects caught may not be useless, as they may be to many other plants (tobacco, for instance) with sticky glands, in which Mr. Darwin could detect no power of absorption. The prevalent idea, that glandular hairs in general serve merely as secreting or excreting organs, and are of small or no account to the plant, must now be reconsidered. Those of the common Chinese Primrose (*Primula Sinensis*) although indifferent to animal infusions, were found to absorb quickly both the solution and vapor of carbonate of ammonia. Now, as rain-water contains a small percentage of ammonia, and the atmosphere a minute quantity of the carbonate or nitrate, and as a moderate-sized plant of this primrose was ascertained, (by estimate from a count on small measured surfaces by Mr. Francis Darwin) to bear between 2½ to 3 millions of these glands, it begins to dawn upon us that these multitudinous organs are neither mere excrescences nor outlets, nor in any just sense insignificant.

Mr. Darwin next investigates the densely crowded short glandular hairs, with their secretions, which form the buttery surface of the face of the leaves of *Pinguicula*, the Butterwort. He finds that the leaves of the common Butterwort have great numbers of small insects adhering to them, as also grains of pollen, small seeds, &c.; that most substances so lodged or placed, if yielding

soluble matter to the glands, excite them to increased secretion; but that if non-nitrogenous the viscid fluid poured out is not at all acid, while if nitrogenous it invariably has an acid reaction and is more copious; that in this state it will quickly dissolve the muscles of insects, meat, cartillage, fibrin, curds of milk, &c.; that when the surface of a plane leaf is fed, by placing upon it a row of flies along one margin, this margin, but not the other, folds over within twenty hours to envelope them; and when placed on a medial line, a little below the apex, both margins incurve. He concludes "that *Pinguicula vulgaris*, with its small roots, is not only supported to a large extent by the extraordinary number of insects which it habitually captures, but likewise draws some nourishment from the pollen, leaves, and seeds of other plants, which often adhere to its leaves. It is therefore partly a vegetable as well as an animal feeder." The leaves in one or two other species were found capable of greater and more enduring inflection, and the glands excitable to increased secretion even by bodies not yielding soluble nitrogenous matter.

The aquatic type of this family is *Utricularia;* and the bladder-bearing species of this genus are to *Pinguicula* nearly what *Aldrovanda* is to *Dionœa* and *Drosera*—the bladders imprisoning minute aquatic animals, by a mechanism almost as ingenious as that of *Dionœa* itself. Observations of the same kind were made in this country by Mrs. Treat, of Vineland, New Jersey, before Mr. Darwin's investigations were made known. These submerged aquatic stomachs, ever deluged with water, apparently do not really digest their captures, but merely absorb the products of their decay.

The same must in all probability be said of such Pitcher-plants as *Sarracenia* and *Darlingtonia*, which Mr. Darwin merely alludes to at the close of his volume but does not treat of. *Nepenthes*, however, according to Dr. Hooker's investigations, has attained a higher dignity, and converted its pitcher into a stomach. This parallelism, and this higher and lower mode of appropriating organic products by each of the three well-marked carnivorous families of plants, are highly suggestive.

In concluding this notice of a book for which we have no room to do justice—but which is sure to be in the hands of many interested readers—there is somewhat to be said in regard to the discovery of the lure in some of our Sarracenias. We have by degrees to discover our discoverers. In this Journal, only so far back as the number for August, 1873, is a notice of the discovery of a sweet secretion at the orifice of the pitcher of *Sarracenia flava*, by Mr. B. F. Grady, of Clinton, North Carolina (in the article by an oversight called "Mr. Hill"), which effectively lures flies to their destruction. This statement, made in a letter, had been for several months in our hands, awaiting the opportunity of confirmation, when an allusion to the same thing appeared in the English edition of LeMaout and Decaisne's System of Botany, without reference to any source, and on inquiry we learned that the authority

for the statement was forgotten. But early in the following year, when the monograph of the order appeared in the last volume of DeCandolle's Prodromus, a reference was found to a paper by Dr. Macbride in the Transactions of the Linnæan Society. His observations (made upon *S. variolaris*), it appears, were communicated to Sir J. E. Smith, read before the Linnæan Society in 1815, and published soon after. They are referred to by his surviving friend and associate, Eliott, in his well-known work, and therefore need not have gone to oblivion, or needed rediscovery here in our days by Mr. Grady and Dr. Mellichamp, the latter greatly extending our knowledge of the subject. Probably the main facts were all along popularly known in the regions these species affect, and where their use as fly-traps is almost immemorial. But the *gist* of these remarks is, that a colleague has just called our attention to an earlier publication than that of Dr. Macbride, viz., an article on "Certain Vegetable Muscicapæ," by Benjamin Smith Barton (one of our botanical fathers), published in Tilloch's Philosophical Magazine, for June, 1812. Among other matters not bearing directly upon this point, he says of *Sarracenia*, without reference to any particular species: "A honeyed fluid is secreted or deposited on the inner surface of the hollow leaves, near their *faux* or opening; and this fluid allures great numbers of the insects which they are found to contain into the ascidia."

Here is earlier publication by three years. Yet we suspect that Dr. Barton knew little about it at first hand, and we find clear evidence that he had not anticipated Dr. Macbride. All his references have an indefiniteness quite in contrast with Dr. Macbride's narrative; he says that "some if not all the species of the genus appear to possess a kind of glandular function," without mentioning those that have it, or the absence of it in the only species growing around him at the north; and he adds that he "was entirely unacquainted with this curious economy . . . when I published the first edition of my Elements of Botany, and even when I printed the appendix (in vol. i) to the second edition of this work." Now his paper is dated September 11, 1811; and the volume referred to, as just printed, is dated 1812. But Macbride states that his observations were chiefly made 1810 and 1811; he corresponded intimately with Eliott, through whom, if not directly, his observations would probably find their way at once to the Philadelphia naturalists. A. G.

3. *The Movements and Habits of Climbing Plants;* by CHARLES DARWIN. Second edition, revised, with illustrations. London: Murray. 1875. pp. 208.—This most interesting treatise was read to the Linnæan Society over ten years ago and published in the ninth volume of its Journal, in 1865. There was a separate issue, which has long been exhausted. It is now carefully re-edited, considerably added to, and reproduced as an independent volume. It will no doubt be much sought after, as the topic and treatment of it are peculiarly fascinating and instructive, and the

book is throughout readable. Mr. Darwin's gift for making things clear without technicalities, is as great as that of many writers for enveloping them in technical obscurity. Having given an account of this essay upon its original appearance, we need only mention the republication, which will be within the reach of all, as an edition is about to be issued by Appleton & Co. A. G.

4. *Hœckel's Ziele und Wege der heutigen Entwickelungsgeschichte.*—The controversy carried on by Hæckel in defence of some of his pet theories has gradually assumed a more and more personal character. The criticisms in his *Generale Morphologie* were sharp, but justifiable from his standpoint. In the *Schöpfungsgeschichte*, they had already become sensational. In the *Anthropogenie* his sketches of contemporaries and his analysis of their work assumed a still more unpleasant emphasis; and this has now culminated in a pamphlet entitled "Ziele und Wege der heutigen Entwickelungsgeschichte."

It is difficult to characterize this production without indulging in the same style of epithets which Hæckel uses so freely. From the title we expected one of those brilliant chapters, which, however untrustworthy, are full of suggestions; we were sadly disappointed to find it filled simply with abuse of His, Gœtte, Ludwig, Reichert, Michelis, Agassiz and others.

We shall not fill the pages of this Journal with countercharges or explanation; a man so skilled in coarse invective, who has risen to such a height of intolerance, is proof against anything so tame as fact or argument. This is not the place to refute his absurd claims to omniscience, and his assumptions of immunity for the very offences he so mercilessly condemns. According to Hæckel it is an unpardonable sin for His or Gœtte to give a false interpretation of what they have seen, or for Ludwig and Reichert to differ from him in his explanation of protoplasm; but when he himself, to suit a purpose, deliberately falsifies facts, when he manufactures with names and figures an archetype which never existed, we are called upon to be grateful that a corner of the veil shrouding creation is lifted, and that we are fortunate enough to live at a time when so infallible an interpreter of its mysteries, has taken up his abode at Jena.

In the concluding pages, devoted to Agassiz and Michelis, all the bitterness of his bigotry and dogmatism are poured forth against the latter, while he stoops so low in his attacks on the former as to pick up all the baseless slanders ever circulated by his enemies during his life. With scientific productions like these we have no concern. A few more such criticisms, and Hæckel's claim to be recognized as a true and devoted student of nature will be forgotten. In its place, he will gain, what he seems to seek, the front rank among scientific demagogues. A. AG.

5. *Memoirs of the American Association for the Advancement of Science.* I. *Fossil Butterflies;* by S. H. SCUDDER. 99 pp. 4to, with three plates. Salem, 1875.—The sum of one thousand dollars was given in Aug., 1873, by Mrs. Elizabeth Thompson, of

New York City, to the American Association, to be used, according to the directions of the Standing Committee, for the promotion and publication of original investigations by members of the Association.

The memoir by Mr. Scudder is the first paper published by the Thompson Fund, and is one which well deserves so prominent a place. Mr. Scudder has had especial advantages in this work, having with one or two trifling exceptions, as he states, "either personally inspected all the fossils described within recent times as butterflies, or having procured new and excellent original drawings of them." He has brought together in this volume all that has been published on this group of fossils whether of text or illustration, presenting thus a complete account of our knowledge of these insects. After the detailed descriptions of the genera and species of fossil butterflies, the author discusses various related topics; their comparative age, the probable food plants of Tertiary caterpillars; the present distribution of butterflies most nearly allied to fossil species, and so on. The plates were executed in Paris, and are beautiful examples of the best lithographic work.

IV. Astronomy.

1. *Small Planets recently discovered.*—In the number of this Journal for August last (p. 158), a table of the planets so far as No. 146 was given. Nine planets have been since discovered, making fifteen during the year.

No. 147	was discovered	by	Schulhof, at Vienna, July 10th.
148	"	"	Prosper Henry, at Paris, Aug. 8th.
149	"	"	Perrotin, at Toulouse, Sept. 21st.
150	"	"	Watson, at Ann Arbor, Oct. 19th.
151	"	"	Palisa, at Pola, Nov. 1st.
152	"	"	Paul Henry, at Paris, Nov. 2d.
153	"	"	Palisa, at Pola, Nov. 2d.
154	"	"	Prosper Henry, at Paris, Nov. 4th.
155	"	"	Palisa, at Pola, Nov. 8th.
156	"	"	Palisa, at Pola, Nov. 22d.
157	"	"	Borelly, Marseilles, Dec. 1.

It has been suggested by Tietjen that No. 152 may prove to be Dike (99). If so the later numbers will need to be changed to correspond. H. A. N.

2. *The Cape Catalogue of* 1,159 *Stars, deduced from observations at the Royal Observatory at the Cape of Good Hope, under the superintendence of* E. J. Stone.—The Royal Observatory at the Cape of Good Hope was established in 1820. The leading idea was to found a first class observatory in the southern hemisphere for work of a character similar to that of the Greenwich Observatory in the northern hemisphere. The observations were to be made with instruments of the same class, and the result to be drawn up in the same form, in order that the whole might constitute two corresponding series, capable of comparison in all their parts. No opportunity of making observations capable of improving our knowledge of the refraction of the atmosphere, was to be

neglected. Under the successive superintendency of Fallows, Henderson, and Sir Thomas Maclear, the latter of whom arrived at the Cape in 1834, and of E. J. Stone, who arrived in 1870, the work of the Observatory has been carried on under very adverse circumstances, and the results thus far accomplished have somewhat disappointed the earlier expectation, and compare unfavorably with those achieved by the energies of Gilliss and Gould. Mr. Fallows was able to publish a small catalogue of star places; Mr. Henderson was able to detect the parallax of *Alpha Centauri*, and to produce a very valuable catalogue of very accurate places of a number of stars. Sir Thomas Maclear seems to have concentrated his energies during many years upon the measurement of an arc of the meridian, of the value of which work there can be but one opinion; but this was allowed to disorganize the other work of the observatory to such an extent that, as Mr. Stone states, he in 1870, found himself with a very limited staff, unexpectedly confronted with the results of 36 years of miscellaneous observations in all stages of reduction, nothing completed, and nothing available for publication and use, without a considerable expenditure of time and labor. Under these circumstances, he has judged it best to pay especial attention to the later years of observation, and has compiled a catalogue of places of 1,159 stars observed in the years 1856 to 1861; all of them made with transit circle, an instrument similar in all respects to the Greenwich instrument, which has been in use since 1851. The Cape Catalogue of Mr. Stone, is accompanied by a comparison of the right ascensions of the clock stars as observed at Greenwich and the Cape of Good Hope, by means of which comparison some systematic errors are brought to light, which are, however, very small in extent, and may be themselves attributed to the effect on the clock of rapid changes of temperature in the evenings during December, January, and February. The latitude of the observatory must, he thinks, still be considered as uncertain.

The printing of the work, which was done at Cape Town, does not suffer by comparison with similar work in England.

C. ABBE.

3. *Observatory in the Pyrenees.*—An observatory has been established on the Pic de Midi, similar to that on the Puy de Dome, and chiefly through the efforts of General Nansouty.—*L'Institut*, Dec. 1.

IV. Miscellaneous Scientific Intelligence.

1. *Reports on the Meteorological, Magnetic, and other Observations of the Dominion of Canada for the calendar year, ending December* 31, 1874.—In this volume the Minister of Marine and Fisheries, Honorable A. J. Smith, has given in full a reprint of the tri-daily simultaneous observations made at a large number of stations throughout the Dominion of Canada, together with tables of monthly and annual means, resultant direction, and velocity of the wind, etc. In consideration of the exceedingly small annual

appropriation at the disposal of Professor Kingston, Superintendent of the Meteorological office of the Dominion, it would seem that the contributions to meteorology by that State are highly creditable. Prof. Kingston states that regular weather reports are telegraphed from fifteen Canadian stations to the Weather Bureau at Washington, in exchange for which a few reports are sent from the United States by telegraph, and a large number by mail. The only station in Canada at present furnished with self-recording apparatus is that at St. Johns College, at Winnipeg, where, by private munificence, an anemograph has been set in operation by the Bishop of Ruperts Land. With reference to Montreal, which ranks as one of the chief stations, it is stated that the position of McGill College, on account of its proximity to the mountains, is singularly ill adapted for anemometric observations, on which account an anemometer was, in August, 1874, erected on a pole on the summit of the mountain, and connected by telegraph wires with the recording apparatus in the Observatory which is 550 feet lower down on the mountain. There are in Canada thirty-five stations to which storm warnings are occasionally forwarded from Toronto. It would appear that these storm warnings do not give so much satisfaction in the Canadian ports as do the corresponding ones in the United States; this deficiency is explained by Professor Kingston as due in an important degree to the errors of incompetent observers, or to the failure in the prompt delivery of reports to the central office, but perhaps especially to the neglect of the agents at the drum stations to report the results of all storm warnings; and he makes a suggestion, which it would be highly desirable to carry into effect in the United States, and, indeed, in all countries, to the effect that all light-house keepers, and all other government officials at inland places, as well as on the coast, be required, as a part of their regular duty, to report promptly by mail, in a very brief manner, the circumstances attending any gale that may occur in their neighborhood. Among the appendices to Professor Kingston's report, is the annual report of the Director of the Observatory at Quebec, who states that the new observatory and house were finished early in May, and the instrument, etc., were removed thither. This observatory contains rooms for the equatorial, the transit and computing room and photographic, and is directly adjacent to the dwelling of the Director, Commander E. D. Ashe. The principal work of the equatorial consists in taking photographs of the sun's surface from which to determine the time of rotation, inclination of its axis, etc. The principal routine work of the observatory, in a commercial point of view, is to give the correct time to the shipping. The time ball is now dropped by electricity, and the method has been brought to a considerable state of perfection. Commander Ashe has also entered heartily into the work of determining latitudes and longitudes of points in the Dominion. C. A.

2. *Mt. St. Elias.*—The recent measurements of Mr. W. H. Dall, Acting Assistant U. S. Coast Survey, of the height of Mt. St. Elias, make it 19,464 feet. The memoir,—a part of the Coast Survey

Between Yokohama and Honolulu the depth is remarkably uniform, averaging 2,858 fathoms, and the material of the bottom is Report for 1875—is illustrated by a map and also a plate containing two views of the Mountain. The views, taken at distances of 53 and 24 miles, evidently have the vertical scales very greatly increased, as compared with the horizontal, but how much is not stated.

3. *Sea-bottom and Zoology of the deep sea: the Challenger's Observations;* by Wyville Thomson.—A gigantic Hydroid was obtained June 17th, in the North Pacific, 34° 37′ N., 140° 32′ E., at a depth of 1,875 fathoms, where the temperature was 1°·7 C. and the bottom gray mud. The species seemed to belong to *Monocaulon* of Sars, a Corymorpha-like solitary polyp; it measured from tip to tip of the expanded tentacles 9 inches, and the height of the hydroid was 7 feet 4 inches. Another was taken July 5, in 37° 41′ N., 177° 4′ W., at 2,900 fathoms, the bottom red clay, but with manganese nodules, the weight of which tore the trawl. The hydroid is too delicate in texture to bear the rough change from the bottom to the surface. The tentacles of the proximal range are about 100 in number and 4 inches long. The sporosacs are in close tufts at the base of the tentacles. This gigantic *Corymorphoid* was associated on June 17th, with Ophidoids, Macrurids, Scopellids, several Gasteropods, Crustaceans related to *Dorippe*, *Galathea*, *Caridids*, and a fine *Scalpellum*, a few Annelids, many Echinoderms (*Brisinga*, *Phormosoma*, *Ophiurids*, *Holothurids*), and on July 15th, there were some *Aphroditids*, a sea-urchin related to *Diadema*, Holothurids, sponges.

The clayey material of the bottom, brought up June 17, was in a peculiar concretionary state, and bored by an Annelid of the Aphrodite group, some of which were still in the burrows.

In a sounding of June 28th, of 2,800 fathoms, a Rhizopod-like form was obtained, between the Radiolarians and the Foraminifers, its test siliceous as in the former, but the shape as in the latter; their tests were extremely abundant in the "red-clay." There were also obtained a *Scalpellum*, a number of Annelids, Echinoderms of the genera *Pourtalesia*, *Archaster*, *Brisinga*, *Antedon*, a *Cornularia*, specimens of *Fungia symmetrica*, some *Actiniæ*. On July 2d, in 2,050 fathoms, the bottom was a light brownish ooze, with many *Globigerina* shells; several specimens of an undescribed *Hyalonema* were brought up.

The cold water which fills up the trough of the Pacific is regarded by Professor Thomson "an indraught from the Southern Sea," as in the Atlantic; and in both oceans the bottom water is constantly moving northward. The temperature of the water for the first thousand fathoms in the Pacific, in the corresponding latitude of 35° N., is much lower than in the Atlantic. Further, in the Atlantic the temperature sinks gradually, though very slightly, through the last thousand fathoms to the bottom, while in the Pacific, the minimum temperature of 1°·7 C. is reached at a depth not greater than 1,400 fathoms, and from that depth to the bottom the temperature is the same.

"red clay," somewhat grayer than the typical "red clay," containing some pumice, numerous siliceous shells, the proportion of which increases with the depth, and scarcely a trace of carbonate of lime (although the water swarms with "ooze-forming" Foraminifers). The pumice was often penetrated with peroxide of manganese, and concretions of the same oxide were abundant in the "red clay." These concretions are rounded or mammillated, fibrous-concentric in structure, and often have a nucleus of some foreign body, as pumice, a shark's tooth, or some other organic relic; and in one case a fragment of a Hexactinellid sponge was preserved as a beautiful fossil at the middle. "The singular point is the amount of this manganese formation and the vast area which it covers." Life was found to be, "although not very abundant in species by no means meagre," in the North Pacific at depths between 2,000 and 3,000 fathoms, all the larger invertebrate groups being represented. In one dredging, at a depth of 3,125 fathoms, a small sponge was obtained, a species of *Cornularia*, an *Actinia*, an Annelid in a tube and a Bryozoon. "We were again struck with the wonderful uniformity of the fauna at these depths—if not exactly the same species, very similar representatives of the same genera existing in all parts of the world." —*Extracts from articles in Nature of Oct.* 28 *and Nov.* 25.

The Challenger arrived at Valparaiso November 19th, on her way home.

4. *Report of an Expedition up the Yellowstone River, made in* 1875; by Lt. Col. J. W. FORSYTH and Lt. Col. F. D. GRANT, under the orders of Gen. P. H. SHERIDEN. 17 pp. 8vo, with a map. Washington, 1875.—This expedition succeeded in navigating the Yellowstone River to a distance of 483 miles above its mouth, the only obstacles to farther progress being the excessively rapid current. It was found that the water of the Yellowstone is deeper than that of the Missouri, above the point where the two rivers join. Some interesting views accompany the report, and also a large map of the river, by Lieut. R. E. Thompson.

5. *Preliminary Report of Explorations in Nebraska and Dakota in the years* 1855, '56, '57; by Gen. G. K. WARREN, U. S. A. 125 pp. 8vo. Washington, 1875.—This is a reprint of the report of Gen. Warren, originally published in 1858, and noticed in this Journal II, xxvii, 378. The present volume is issued in view of the general interest now felt in the Black Hills country, the original report being practically inaccessible.

6. *Atti della Societa Toscana di Scienze Naturali Residente in Pisa.* Vol. I. Parts 1 and 2. 146 pp. roy. 8vo. Pisa. 1875.—These first publications of the Tuscan Society of Science in Pisa, contain papers on the mammalian fauna of the Pliocene of Tuscany, by C. I. F. Major; on the fishes of the same by R. Lawley; on Eocene corals of Friule by D'Achiardi; on the natrolite (savite) and analcite of Pomaja, by D'Achiardi, and other papers geological and zoological, by Meneghini, De Stefani, Baraldi, Richiardi, with one botanical, Sulla teoria Algolichenica, by G. Arcangeli.

OBITUARY.

EMILE KOPP, Professor of Chemistry in the Polytechnic School of Zurich, died on the 30th of November at the age of fifty-nine years. He was an Alsatian by birth, and held a chair in the University of Strasbourg previous to 1848. He took an active part in the revolution of that year, and was one of the Deputies who escaped to Switzerland at the time of Louis Napoleon's *coup d'etat.* While residing in Switzerland he was appointed Professor of Chemistry at Lausanne, but he left the country voluntarily, with the other French exiles, when their rendition was demanded by the French government. Passing into England, Kopp supported himself for several years as a private tutor at Manchester, and at the same time familiarized himself with the great chemical industries of that vicinity. The influence of his sojourn in England was strikingly manifest throughout his subsequent career. After the lapse of several years he was permitted to return to France on the parol of one of the Senators of that period (probably M. Dumas) who pledged himself that the returned exile should in no way interfere with the imperial government. On reaching Paris, Kopp opened a private laboratory for instruction in applied chemistry, which was maintained for several years, and was always filled with students. From this laboratory he was called to the charge of extensive works for the manufacture of steel at Saverne, in the east of France, which place he left some years later to assume the chair of applied chemistry in the University of Turin, whence he was soon called to Zurich.

For many years Kopp exhibited great literary activity, and he is probably best known to the generality of chemists from his remarkable compilations relating to the history and progress of the coal-tar colors and of the madder colors. He was largely instrumental in writing Hofmann's famous report on the Chemical Products and Processes of the International Exhibition of 1862, as was duly acknowledged by Prof. Hofmann. This report, as is well known, has served as a model upon which most subsequent reports upon chemical matters have been based. But, in spite of much writing, he accomplished a great deal of work in the way of research, notably in respect to the coloring matters just mentioned, and in other departments of calico-printing. He devised novel processes for making soda from salt, and for the recovery of sulphur from soda-waste, and published numerous observations upon a great variety of subjects.

His familiarity with the methods and processes of technical chemistry, as applied in different countries, was very great, and his judgment of them was singularly sound and impartial. He labored untiringly to inform himself of all improvements and discoveries in the domain of chemical technology, and was doubtless at the time of his death one of the best teachers of applied chemistry that has ever lived.

WHEATSTONE.—Sir Charles Wheatstone died at Paris, on the 19th of October, at the age of seventy-three years.

STORM OF FEB 5-8 1870

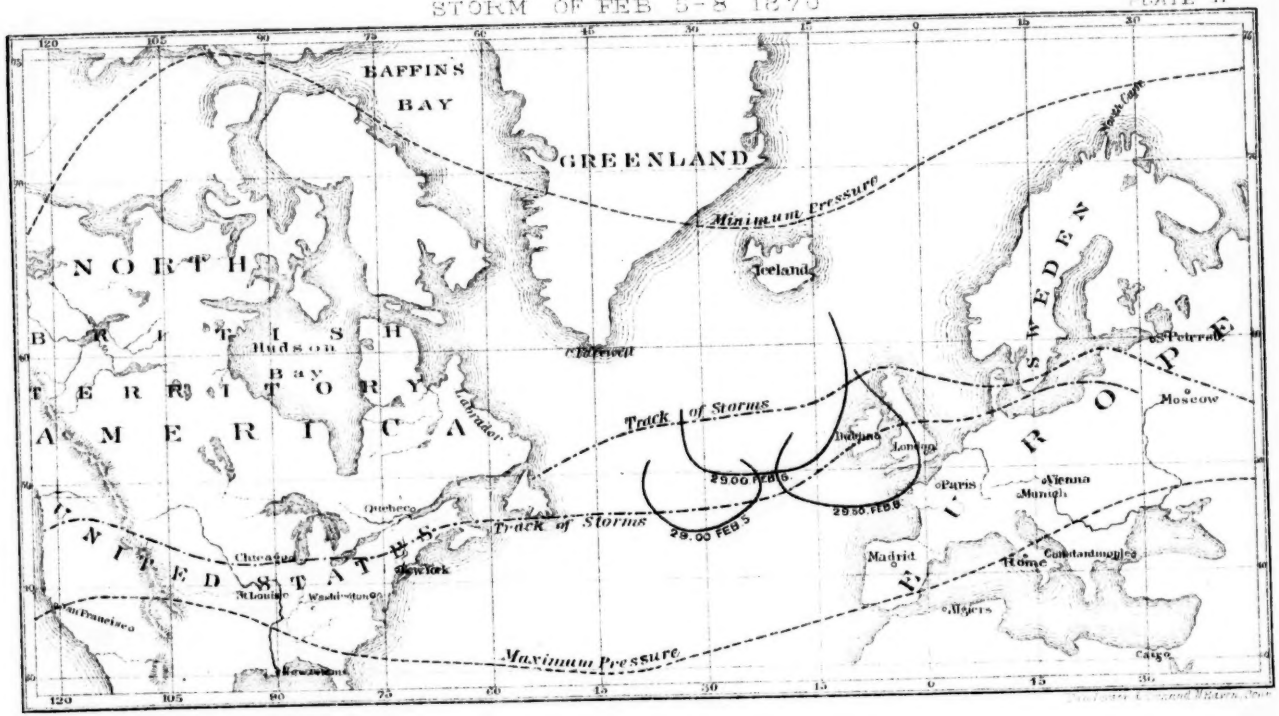



PLATE I.